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A Guide to

extensive testing on farms

in four parts

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PARTS I and II.

Foreign Agricultural Service April 1954
U.S. DEPARTMENT OF AGRICULTURE

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Actual farms—with their various soil, slope, cropping, and management conditions—are the ultimate test of any new or improved practice.

a guide to

EXTENSIVE TESTING ON FARMS

by Henry Hopp

in 4 parts

Part I: Introduction

Part II: Result Tests

Foreign Agricultural Service
UNITED STATES DEPARTMENT OF AGRICULTURE
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PART I. INTRODUCTION

> "Farmers generally would not change their practice from observing what could be done on farms operated at public expense. There must, therefore, be demonstrations carried on by farmers themselves on their own farms and under ordinary farm conditions."
>
> —A. C. True.
>
> Like the farmer, the technician also demands demonstration: before he recommends a practice, he wants proof that it is applicable to the farms in his area.

Since World War II the concept of technical service to farmers as a tool for agricultural progress has grown rapidly in acceptance throughout the world. Programs to give this type of service are being set up at an unprecedented rate. Naturally these programs look to older ones for guidance.

The older technical-service programs generally operate through two well-recognized organizational entities: An experiment station and an extension service. In its simplest form, the experiment station is a place at which experiments are conducted to develop better practices for farms, and the extension service is a group of advisers who help farmers adopt these practices.

But these definitions tend to obscure a vital step in the process of agricultural advancement: the whole complex of testing and proving operations that goes far beyond the intensive research at the experiment station itself and is necessary before an improved agricultural practice is ready for recommendation to farmers. This complex involves outlying substations, experimental farms, experimental plots on farms, farmer cooperators, and often commercial organizations such as seed companies and farm-implement manufacturers. In fact, a considerable part of the work of experiment stations consists of the extensive testing that is carried on outside the physical confines of a main station.

Each of the two levels of testing serves a distinct purpose: intensive research, at the experiment station, discovers a good practice; exten-

sive testing, on farms, determines the applicability of that practice over a region. Intensive research gives the first clues as to what is a better practice but usually is not a sound basis for a flat recommendation to farmers. Only after an extension agent is armed with satisfactory information on applicability, can he confidently recommend a practice, knowing in advance what results farmers should get.

How extensive testing works in an agricultural-service program is illustrated by the cooperative activities in California. The foreword to Research Program of the Experiment Station, 1950-1951, published by the University of California's College of Agriculture, states, "The Agricultural Extension Service and the Agricultural Experiment Station have over 7,000 tests, demonstration and research plots scattered over the state each year. They work closely together." And J. Earl Coke, formerly director of agricultural extension in California and now assistant secretary of the United States Department of Agriculture, wrote to the author on September 2, 1952, as follows—

> "Because of the variety of climate, soils and crops grown in California, it is necessary to conduct test plots in the field to determine actual adaptability of crops under local conditions. Therefore, for many years the Extension Service has conducted many tests, in most cases with the cooperation of the Experiment Station. For example, grain variety trials are conducted throughout the State by the farm advisors. In the majority of the cases the seed for these variety tests comes from the Experiment Station, which also threshes, weighs, and tabulates results from many of the plots. In addition to the agricultural extension test plots, field tests are conducted by the Experiment Station in various counties, in which in most cases the farm advisors are involved in some way. The farm advisor may assist in the selection of the cooperator and the plots; he may also assist in the application of various treatments used in obtaining the results. This works very much like the tests run by the Extension Service except that the leadership comes from the Experiment Station."

We see, then, how intensive research at an experiment station is not enough support for an extension program and must be reenforced by adequate extensive testing. Herein lies guidance for organizers of a new agricultural-service program. Although at the beginning the organization will have a much smaller extensive testing program than the State of California, it should nevertheless recognize the place that each of

three steps has in agricultural development: (1) Intensive research at the experiment station to develop practices, (2) extensive testing of these practices to determine applicability under farmers' conditions, and (3) assistance to farmers in putting the practices into effect. Failure to provide for the second step tends toward research without opportunity for practical outlet, and toward extension without a proved technical basis.

This Guide has been prepared to aid the agricultural technician who is specifically concerned with extensive testing on farms. When a new practice is suggested to him, either by the experiment station or by his own experience, he must still find answers to several questions before he can recommend that practice: How will the practice work in the different parts of my area? How must it be modified for farm use? Will it yield enough benefit to be worthwhile? How much will the benefit vary?

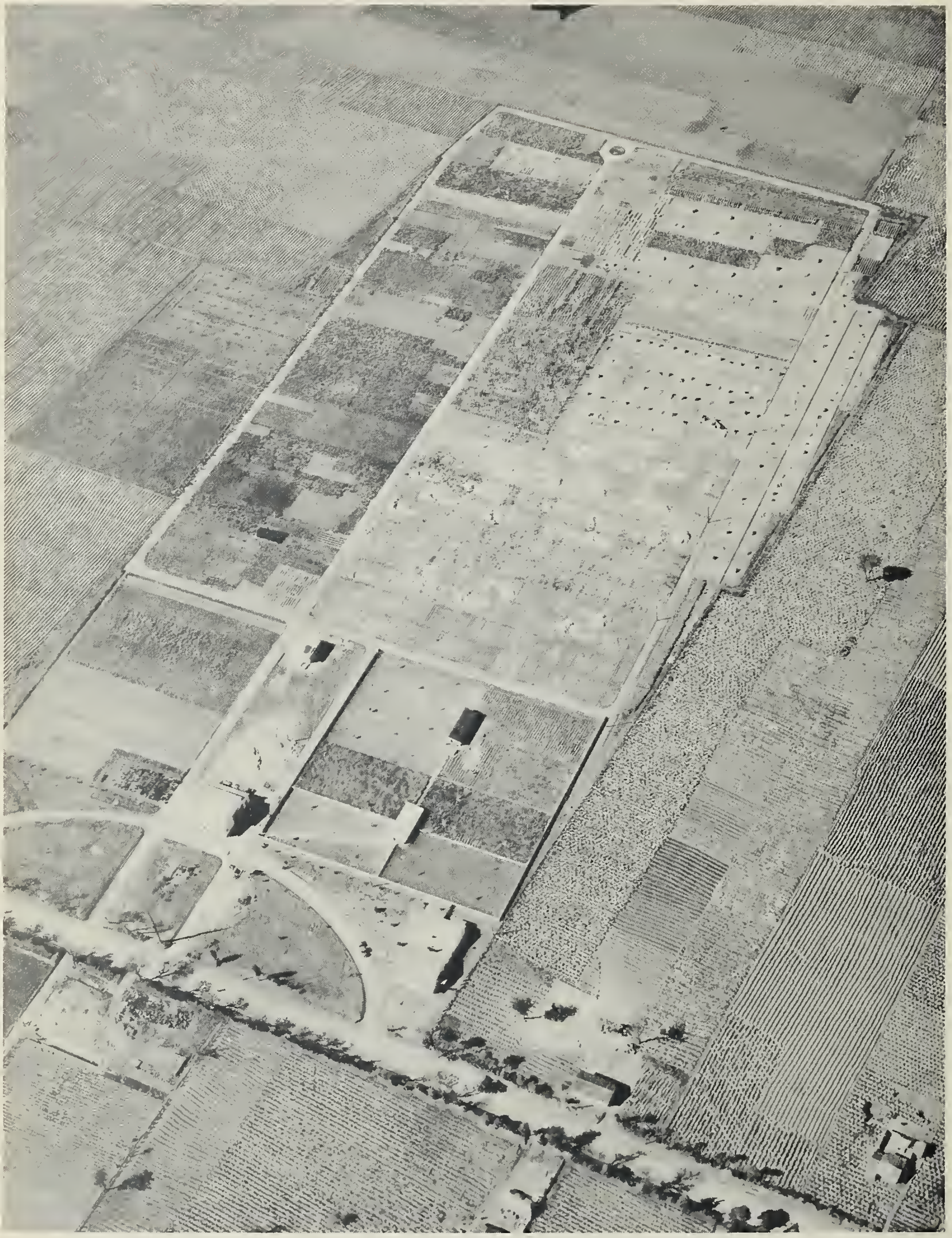
Answering these and similar questions on applicability involves a kind of testing that is different from the intensive research done at the experiment station. For one thing, it requires the cooperation of farmers; for another, it has to be conducted in many locations and under actual farming conditions. Sometimes, to get valid measures of applicability, the technician must go even further than locating the tests on farms: sometimes he must have the farmers themselves do the work, with their own implements and in their own way.

The technician concerned with testing a practice for adaptability finds himself in the intermediate role of researcher and extension man. In fact, since extensive testing is a step that lies between the intensive research at the experiment station and true extension work on the farm, it is a step that can be taken either by the researcher as a sequel to his station experiments or by the extension man as a preliminary to his blanket recommendation of a practice.

WHEN DO YOU MAKE EXTENSIVE TESTS ON FARMS?

The concept of extensive testing requires that you carefully assess the real reason for your test. If you decide specifically how you want to use the information developed by the test, you will usually be able to decide whether you need an intensive test, an extensive test, or possibly both in sequence. When you intend to use the information to make a final recommendation for farmers, you probably need a test of an extensive nature.

In conventional research, the technician is concerned with a number of practices—sometimes a very large number—and with exact measurement of the responses. For these reasons, he generally lays out his tests in replicated, or repeated, plots on one or more experimental areas. In extensive testing the situation is somewhat different: there the technician usually begins with a practice that is reasonably certain



Experimental plots at a research station. Practices proved superior here usually need extensive testing on farms before they can be recommended for actual farm conditions.

to have a beneficial effect, but he is trying to determine its applicability to farms.

To make clearer the distinction between questions answered by research and questions answered by extensive testing, we might point out broad problems that typically involve applicability and therefore are solved by extensive tests on farms and not by experiments at the research station.

To determine conditions in an area.—Some tests are undertaken to determine conditions in an area for which the technician is responsible. An example is a test to determine the average response to fertilizer. Problems of this kind might be looked upon as a survey conducted by means of tests. The answer cannot be obtained through research at one or two research stations; the validity of the answer depends on getting an adequate number of tests over the area.

To find responses for different regions.—Another group of problems concerns different responses to a practice in the different regions of the technician's area. A region may be a soil series, a geographic complex, or even a particular class of farmers. You may have a lead on a practice from research at the experiment station, but you may still need to delineate the regions where the practice applies or where variations need to be made in the practice. For example, in one soil region a 10-6-4 fertilizer mixture may be the best; in another, a 5-10-5. Such a problem cannot be solved at one location; the tests will have to be made at a number of places in each region.

To find responses under actual farming conditions.—Research is often conducted under "ideal" conditions, or at least under conditions not usual on actual farms; and the clue to a better practice thus comes out of nonrepresentative conditions. The problem still remains of determining what results the practice will give on the farm. Response of a new crop variety at the research station, on soil that has been managed well, may be far different from the response on farms, where fertilizer practices might be variable and quite different from those at the station.

Or it may be a question not of differences in land conditions but rather of farmers' operations. Then the testing has to be done not only on the farms but also by the farmers as well—each with his own implements and in his own way. When this latter concept is involved, research tests may indeed be quite inapplicable.

Sukhatme[1] cites an outstanding example in India of a comparison between the results obtained on farms and at experiment stations. His data show the uncertainty of using research-station results as a measure

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1/ T. V. Sukhatme. "Assessment of Additional Food Production" (report of sample survey in Madhya Pradesh, 1949). Agr. Sit. in India 5:719-724, 1951.

of farm response. He found (1) that the land to which farmers applied new practices was much better than the average farm land of the area, and (2) that, despite this fact, farmers' results were much less than anticipated from research-station results. Some causes of such discrepancies between farmer and research-station results are that—

1. Yields from small experimental plots are usually greater than yields from farmers' fields.
2. Cultural operations (land preparation, seeding, cultivating, and harvesting) in research stations are usually more timely in application and are more thoroughly and expertly done than on farms. Besides, some of the farm implements used at the station may be different from those used by farmers.
3. Research personnel usually apply a practice more adequately and with more ample consideration of the factors that produce success than do farmers, who perform the practice as a part of their farming operation.

To measure the profitableness of a practice.—Before a practice can be recommended to farmers, the question of its profitableness often must be answered. For example, you might have this problem: What quantity of fertilizer is the most profitable for farmers to use, considering that there may be many unknown factors, other than nutrient deficiency, that are limiting crop growth under actual farming conditions? The best quantity of fertilizer for farmers to use, year in and year out, may often be less than the quantity that gives the maximum yield response.

To measure the variability of benefit.—In determining whether farmers in a certain region should adopt a practice, you must be concerned not only with the average benefit in the region but also with the consistency of the benefit to the individual farmers. A practice that gives large benefits on some farms but none on others is not as safe to recommend as a practice that gives consistent results everywhere.

To assess a practice when there is no single check.—You know that experiments at research stations often include comparisons with present practice. These comparisons are simple to make when there is a single present practice: the experimenter simply includes a check plot in the test. But sometimes there is no single present practice. For example, if each farmer grows his own corn, the experimenter would need as many checks as there are farms. Or, if farmers have different breeds of cows, a large number of checks would be needed in order to test, let us say, a new feeding practice at the research station. It might then be easier to conduct the tests directly on farms, and the checks will be the different actual practices on the several farms.

HOW LONG SHOULD EXTENSIVE TESTS LAST?

For some practices the results are influenced as much by weather as by soil. A practice that gives consistent benefit in both good years and bad is a safer practice to recommend than one that works only in good years. Therefore, if the consistency of a practice from year to year is in question, you will have to repeat the test for several years.

Some practices are likely to be much influenced by yearly variability in weather; others are not. Thus, large applications of fertilizer may be profitable only in seasons with ample rainfall; and an extensive test to determine the benefit year in and year out would have to be repeated several years before final recommendations could be made. On the other hand, feeding an improved ration to calves is likely to have benefits that are fairly independent of weather; test of such a practice could probably be accomplished in one year, or even less.

If the extensive test has to be repeated for several years, no hard and fast rule can be given as to how many years are required; two years may be enough, but several more may be needed. Your aim should be to run the test for enough years to take in reasonable extremes of weather. Long-term weather records will help you determine when you have struck extremes; but, if such are not available, you can resort to the opinions of extension people and farmers, who have reason to remember the weather.

WHAT ARE THE KINDS OF EXTENSIVE TESTS?

For convenience, it seems advisable to distinguish between two kinds of extensive tests on farms—the result test and the farm experiment.

The result test is undertaken to find the result of applying a single practice under farm conditions; it is simple, requires little knowledge of research methods, and has high demonstrational value for farmers. You are already familiar with a related term, "result demonstration." A result demonstration is used to demonstrate a result; a result test is used to test a result. You perform a result demonstration only after the practice has been proved; you perform a result test before it is proved.

Often, however, you may have to test more than one practice. Then you will have to conduct a more complex kind of test—the farm experiment. This kind is undertaken to find which of several practices—for example, several fertilizer formulations—is best under farm conditions. It requires more application of statistical methods and has less clear-cut demonstration value. The designing of it requires the cooperation of technicians with experience in research.

PART II. RESULT TESTS

> "It has been said that farmers are a hard class to reach and impress. That is not my experience. They are the most tractable of people if you have anything substantial to offer—but they want proof."
>
> —Seaman A. Knapp.

The result test is closely related to the result demonstration, a technique already in wide use by extension workers. The result demonstration helps a community of farmers to learn the benefit of a recommended practice. Its purpose and technique have been summarized as follows:

> "It is used to prove the practical application of basic facts to farm and home problems and is in no sense experimental except possibly in the mind of the demonstrator. With this method the extension worker can utilize the results secured from the adoption of a farm or home practice or a combination of practices to prove by comparison the value of the new method."[2]

The result demonstration and result test are often quite similar when carried out in the field. The reason is obvious: often the same kind of proof may be required to show the farmers the benefit of a practice as is required to show the technicians. Besides, a result test usually has fine demonstrational value; it is an effective tool for teaching as well as for learning.

But the result demonstration, in its strict sense, is used only with practices of which the technician already knows the true benefit. His purpose is to give the farmers enough concrete experience with the practice to convince them of its effectiveness. In the result test the same technique is used: trials are made on farms and results are evaluated. But the primary objective is distinctly different. A result test is under-

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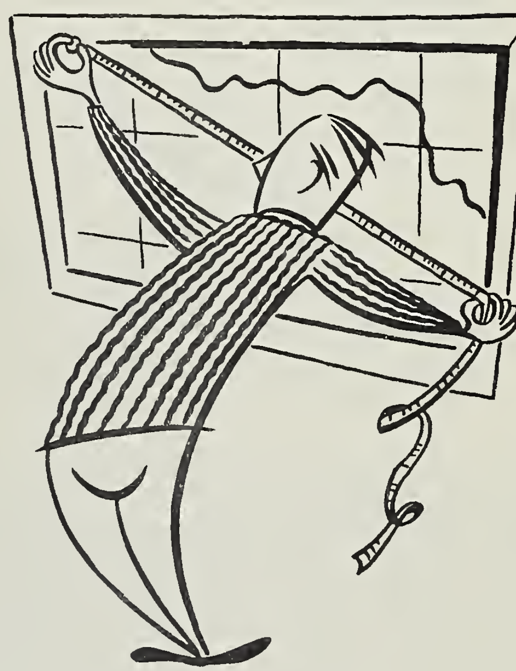
2/ Lincoln D. Kelsey and C. C. Hearne. Cooperative Extension Work, p. 345. Ithaca, N. Y., 1949.

taken to determine the benefit or effectiveness of an improved practice under the farming conditions of the community. The result test precedes issuance of a recommendation; the result demonstration follows it.

In the result demonstration the number and location of trials are decided by the need for teaching impact on the community; in the result test, by the need for measurement of the practice. Usually, though not always, a larger number of trials will be required for a result test than for a result demonstration.

The proper design of a result test is important to assure its validity. We shall therefore point out in distinct steps the procedures you should follow in setting one up.

STEP 1. DELIMIT THE REGIONS. Begin by deciding on the regions within your area to which the practice should apply. This is a point that is easily overlooked; too often we get into the details of making the test before we ever decide on the regions of application.



Of course, if your particular area is one in which approximately the same conditions—of soil, climate, altitude, and so forth—prevail throughout, you will plan to issue only one set of recommendations for the whole area and will therefore have no reason to delimit subdivisions. But if your area is made up of dissimilar regions, each of which may call for a different set of recommendations, you must define these regions.

Generally you will start with a map of the entire area in which you are working. On it you will mark off the regions where the new practice is to be tested, i. e., the subdivisions for which you will issue separate recommendations if results justify. These subdivisions are called test regions. In deciding how many of these to have, you must temper your subdividing with a consideration of practical limitations: The cost of testing in, and the complexity of recommending for, many diverse regions.

Let us see how this is done. The map shows the subdivisions decided upon for a corn-variety test. In this hypothetical example the corn-grow-

ing area was first marked off into two main regions—one in the west and another in the east. These were set up as separate test regions because each had different growing conditions and would doubtlessly call for separate recommendations.

Delimitation of area of application for a proposed practice, and subdivision of the area into test regions (I, II, III, IV).

In the western region two further divisions were made according to altitude—the coastal plains (I) and the mountains (II). This was done because research was finding that not all altitudes favor the same varieties.

Region II contains three soil types, but these were not designated as separate test regions because they were not extensive and because separate recommendations for each soil would be too complicated to follow.

The eastern region was divided according to farming practice—dry farming (III) and irrigated farming (IV). This was done because the varieties that will prove the best under one practice will probably not prove the best under the other and because farmers can easily follow the different recommendations for irrigated and nonirrigated land.

No further subdivisions were made because the number of tests required for these four regions was all that the organization could carry out.

Now, after these four regions were delimited, a complete result test was laid out in each region.

STEP 2. DECIDE ON THE NUMBER OF FARMS. Experience has shown that you should have 15 to 30 farms in a result test. Fewer farms will probably give you insufficient information for a sound recommendation to farmers. On the other hand, more than 30 farms will rarely be necessary for a good appraisal of a practice.

You may wonder why the number of farms is important. "Wouldn't one really good test on a single farm do?" you may ask. A moment's thought will show you how dangerous that idea is.

Look at it this way. The farmers of your area, with your guidance, are trying to determine what benefit they will get from the new practice. You want to know this so definitely that you can issue a clear-cut recommendation to everyone. If you conduct a test on just one farm, even if it is a very good test, it will apply only to that one farm. After all, soils vary a great deal over a region, and farmers vary too. You cannot be sure, from the one farm, what results farmers in general will get. Obviously, you are on dangerous ground in trying to reach a general conclusion from one case.

Then there's another point to bear in mind if you are considering a test on just one farm. The farm you use for the test may show a marked benefit from the practice, may even give you an increase of 50 percent or more. But no matter how promising the results on that one farm may be, they still cannot tell you how much the benefit will vary from farm to farm. The very practice that proved so successful on one farm, may give highly variable results when tried on other farms; and a practice that gives highly variable results is not as safe to recommend to farmers as one that gives consistent results. It is sobering to consider that many a potentially good practice could be thrown completely "out of the running" just because it failed in a one-farm test.

Consider an example in which Practice A and Practice B are both tried on an adequate number of farms. As the following diagram shows, each gives the same average yield, 50 bushels per acre. Judging from that fact alone, one practice seems just as good as the other. But when you examine each practice for consistency of result, we see that Practice

B is obviously the safer one to recommend. The lowest yield we get with B is 40 bushels, but with A we get a yield as low as 10:

Practice A

Lowest yield		Average yield		Highest yield
10 bu.	⟵⟶	50 bu.	⟵⟶	90 bu.

Practice B

Lowest yield		Average yield		Highest yield
40 bu.	⟷	50 bu.	⟷	60 bu.

In other words, if you had made just a one-farm test, both practices might have given the same increase in yield. Here is where testing on a single farm really falls down: it shows absolutely nothing about the variability of the benefit over the region. The only way to determine the variability is to try the practice on a sufficient number of farms.

Although we know from past experience that 15 to 30 farms are required for most tests, these numbers are, after all, a rather offhand specification. To be exact, the number of farms you will require in order to have confidence in the answer from your test will depend on two considerations: (1) How great a benefit you expect from the new practice on the average and (2) how much variability you expect the results to show. If you expect a large benefit from the practice and fairly consistent results throughout the region, you can cut down on the number of farms.

The little table on the next page will help you decide how many farms to have. Suppose, for example, that you want to test a new variety of corn for your region. Let us say that you expect it to give so large a benefit that it doubles the yield, that is, gives an average increase of 100 percent. Let us say, too, that you expect the increase to be quite variable in the region. Then, referring to the table, you will find that 15 farms or so should participate. Of course it is well to put the test on a few extra farms besides, since on some farms the test may not be carried through to the end.

This is about as close as we can come at this point to determining the number of farms that are required. As you might suspect, there are more precise methods for making this determination. But they are rather complex.

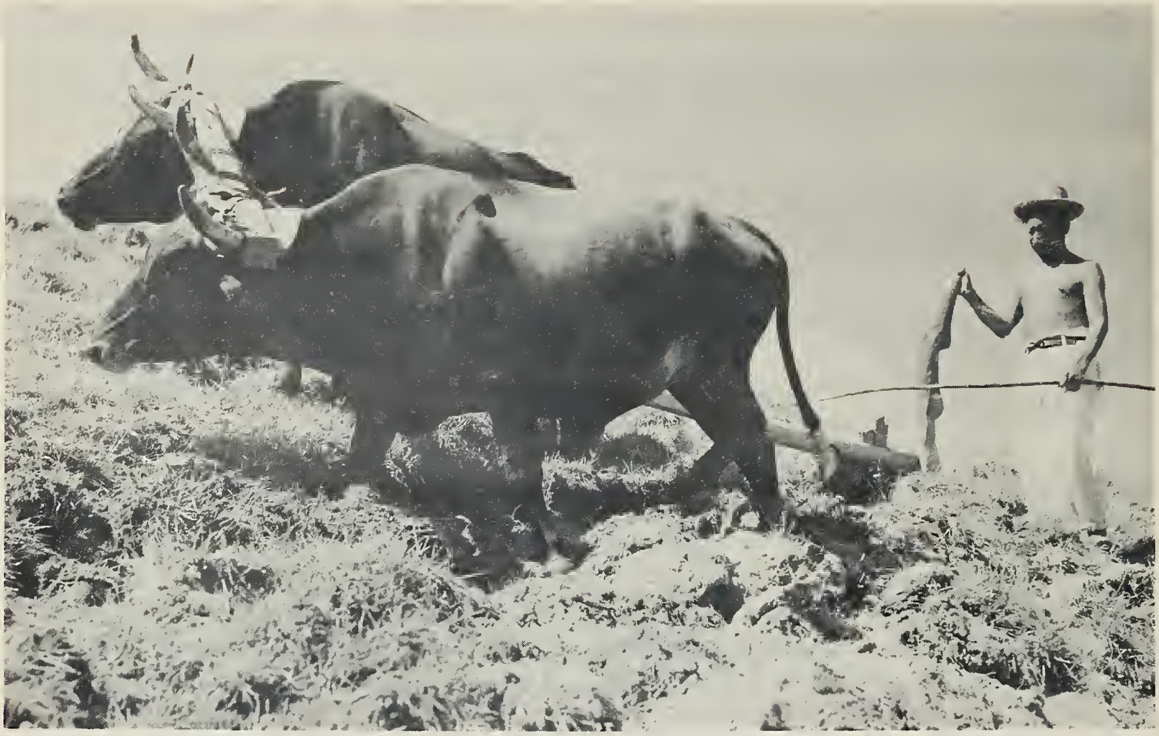
If you expect an average increase of—	And, if you expect the increase in the region to be—	Then you should have this number of farms:
200%	Quite variable	10
	Fairly consistent	7
100%	Quite variable	15
	Fairly consistent	10
50%	Quite variable	25
	Fairly consistent	15
25%	Quite variable	30
	Fairly consistent	20

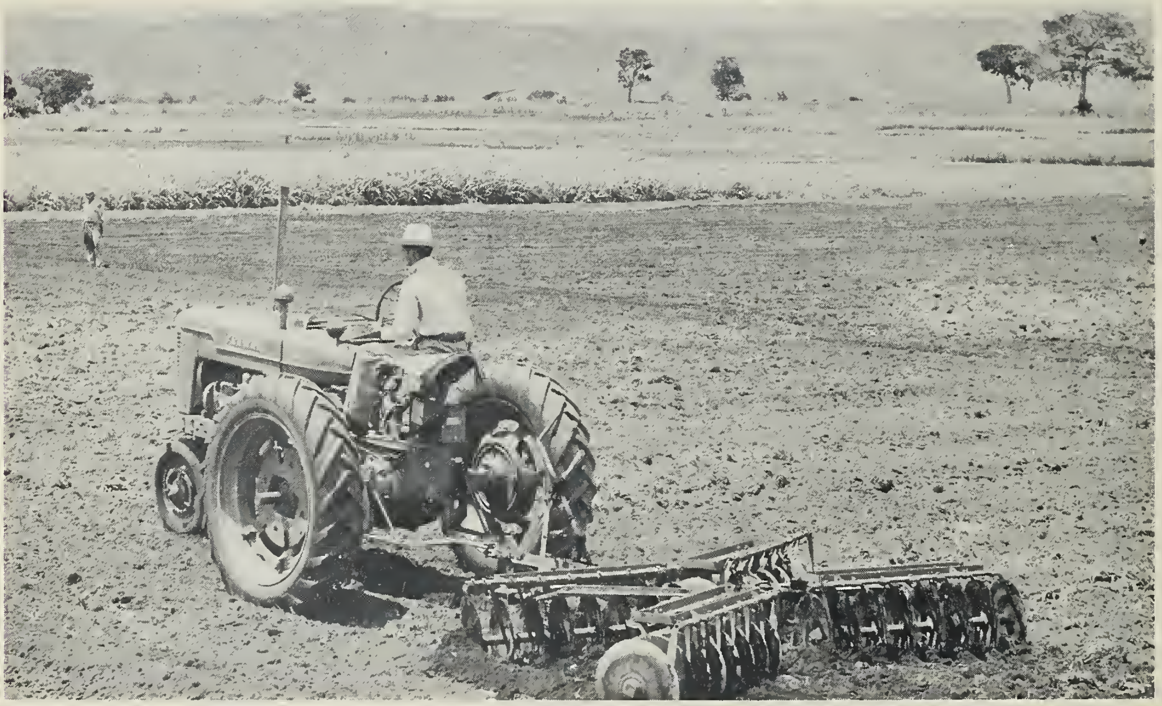
If you ever get into a costly result test, in which it is important to determine the best number of farms quite accurately, you might want to use a more precise method. The procedures are given in Parts III and IV of this Guide. But for most tests the foregoing table is adequate.

STEP 3. SELECT THE FARMS. The purpose of your test is to obtain an answer that applies to all farms in the region. Yet it is obviously impossible to have all farms participate in a result test. The criterion whereby the participating farms are chosen is important: the participating farms must be representative of all the farms. If they are not, you will never get results that are correctly applicable to the region, no matter how carefully you conduct the test.

One method of selecting the farms—a method that is no longer considered valid—is to choose farms that are thought to be "typical" of the region. Sometimes you will hear a technician say, "I selected this farm for the test because it is typical." Get away from that idea, "typical." It is fraught with danger.

First of all, the concept of a "typical" area is fallacious. There is no such place! A region





Modern and primitive cultivating methods practiced in the same community point up the need for tests of applicability. To make sure that results of a test apply to a whole community, the technician must put the test on a representative cross section of all farms.

is certain to include many variations—in topography, soil, previous cropping practice, farm size, method of cultivation, farmers' abilities, and so forth. There is, in truth, no way to define what is typical. No single plot of ground can represent an entire area, with its many diversities.

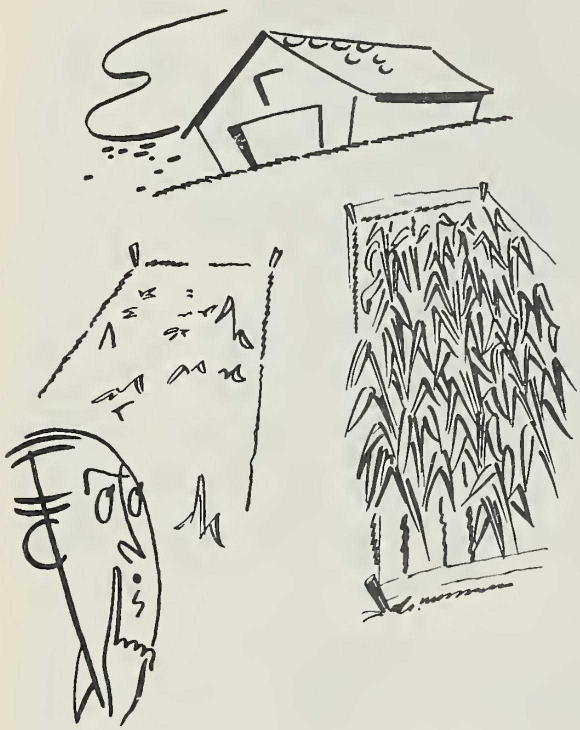
There is another reason for getting away from the idea, "typical." Even if you could select typical farms, you would not be doing anything worthwhile. For then the results of the test would apply only to these typical, or average, farms. But in a result test, not only do you need to know the average benefit of the practice; you need to know also how consistent the benefit is over the range of conditions in the region. To find this out, you will have to set up the test on a range of farms. Be careful not to overlook this point: Variability of the response is just as important as the average of the response.

In view of the nonexistence of typical sites and the need to test a practice on a range of sites, the only valid procedure is to select farms for the test that are a fair cross section of all the farms. Only then will you have farms participating that are a true representation of the farms in the region.

Now, how can you get a fair cross section? The ideal way would be for you to put all the farmers' names in a hat and, blindfolded, to draw out the required number. In practice, however, it is usually impossible to select the farms in so random a manner. For one thing, it may be inconvenient to obtain a list of all the farmers in the area. Besides, it may not be feasible to include certain farms.

In selecting the test farms, then, you will have to compromise on the principle of randomization. You may have to confine them to farms located along accessible roads, or to farms with which cooperative relations can be easily established. The less you confine your choice to a particular class of farms, however, and the more you take the farms "as they come," the closer you will be to having a true representation. Remember this: Every departure from randomization imposes the danger of bias in the applicability of the results. But this is a risk you have to take. It will be up to you to decide how far you can wander from random in selecting the farms without seriously detracting from the applicability of the results to the region.

STEP 4. DECIDE IF YOU NEED CHECK PLOTS. You must now decide whether your test requires one or two plots per farm.



If you intend that farmers should substitute the new practice for some practice they are already using, you should have two plots on every farm: one for the new practice and one for the old. The following are examples: (1) Substituting a new variety for an old; (2) using fertilizer instead of none; (3) using an insecticide instead of not; (4) trying a new ration for cows instead of the old one.

The old practice is called the check, or control. It is the condition against which the new practice is to be compared.

You may note that the term "plot" does not seem to be the one to use for animal tests or tests in the farm family. You can still use the term, however, except that you should realize that in such a circumstance the plots are groups of animals or people, one receiving a new practice and the other receiving the check practice.

Some kinds of comparisons do not permit you to have the check on the same farm as the new practice; for these you will have only one plot per farm. For example, if you are testing the effect of pest control on cattle, you might have to treat a whole herd. Then the new practice is applied to one farm, and another farm serves as the check. Each farm becomes a plot; and two farms serve as a single comparison, just as in the foregoing example, where the two plots were on the same farm.

Sometimes the test is not comparative; the practice under test is not intended to replace what farmers had previously been doing. Possible examples are—

1. Finding out how a new vegetable does in a region.
2. Finding out if rabbit culture pays.
3. Making a survey of yield of wheat.

In such cases you have just one plot per farm. The information obtained from each farm will be the production of the plot rather than the advantage it has over a check plot. Here, again, if the test is with animals or human beings, the word "plot" means the group being tested.

A farmer and his family join the technician in making plans. Extensive testing requires farmer cooperation and, often, farmer participation.

STEP 5. PUT IN THE PLOTS. So far you have decided on the number of farms, selected them, and fixed the number of plots per farm. Now you are ready to start the actual field work of putting in the plots.

Choosing the location of the one or two plots on a farm will be influenced considerably by the desires of the cooperating farmer. He may want to have the plots in a certain field, for example, close to a road. This is all right. There is just one point to remember: Do not select the piece of ground solely because it is the best piece of ground on the farm. Rather, select it without any judgment one way or the other as to its relative fertility. Your aim should be to locate the test plots at random, without any purpose for your choice as far as soil conditions are concerned. Follow this thought in locating the plots on every farm; then the test as a whole will be a true cross section of the conditions in the region. Some plots will be on good ground, some on poor. All in all, you will have a fair representation of the soils in the region.

When your test requires two plots on a farm—a check plot and a new-practice plot—you must be careful to guard against bias. Do not pick the place for the new-practice plot so that it is on more fertile land than the check. Doing so would be an error; it would invalidate the honesty of the test, for the results might show very much in favor of the new practice. The test would be biased and would not show the true benefit of the practice. You will use the results of the test as a guide in making recommendations to farmers, and you do not want the difference in yield between the new and old practices to be biased by differences in soil fertility.

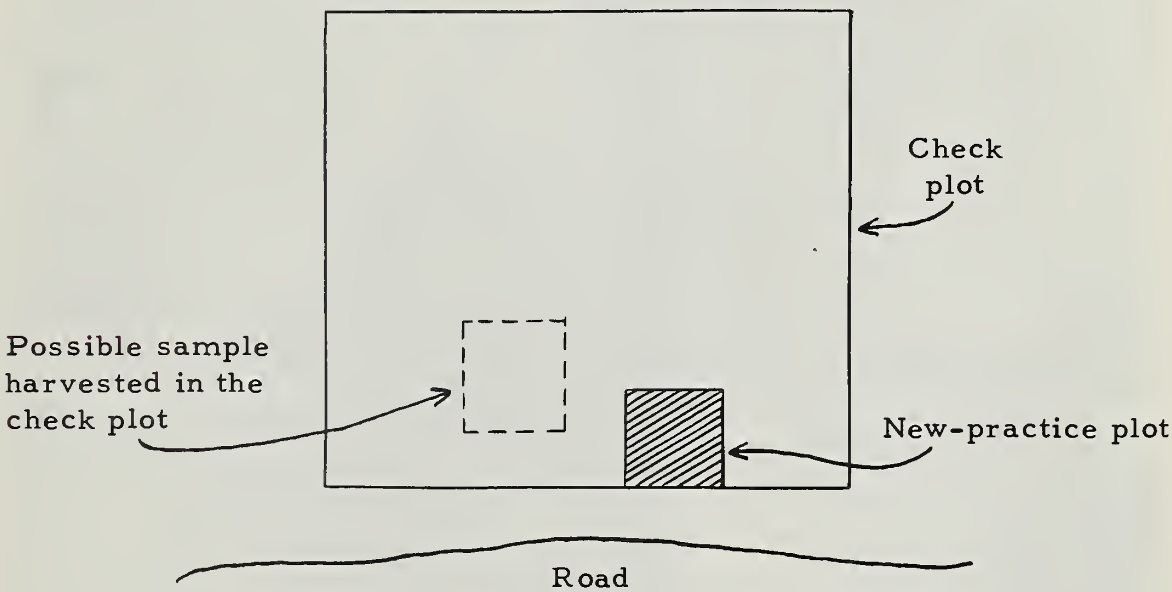
You would like, of course, to have the two plots fall on land that is exactly alike in fertility and other respects. But this coincidence would be quite a difficult thing to accomplish, if not impossible.

Have the plots as close together as possible and choose them according to convenience rather than relative fertility. Rely on repetition of the plots from farm to farm to equalize whatever fertility differences may exist between the plots on each farm individually.

The size of the plots is a further consideration. You know that at

research stations most experimental plots are rather small and all of the same size. But in most result tests you will make the plots large; what is more, you need not have them the same size on all farms. Observe this rule: Make the plots large enough to permit following the usual farming methods and to make the results plainly visible. Anything larger than this is unnecessary, and will not make results more precise.

Moreover, the check plot on a farm need not be the same size as the new-practice plot. As the diagram shows, the new-practice plot can be just a segment of a field, with the remainder serving as the check.



In result tests, the check plot may be much larger than the new-practice plot. Sometimes just a sample of the check plot may be harvested for the comparison.

Then, when harvesting, determine the areas of the two plots and convert the yields to an equal-area basis.

In getting data, it may be more convenient to harvest just a sample out of the check plot. Try to locate the sample next to the new-practice plot and have it about the same size.

If you harvest just a sample out of the check plot, be careful again to guard against bias. Do not deliberately select a poorer spot. The way to avoid bias is to decide on the location of the sample at the very start of the test, before you see the results. For example, you might decide on having the sample of the same dimensions as the new-practice plot and adjoining it on the west.

To be safe, stipulate the position of the sample when you write the test plan—even before you start the field work.

The farmer measures the benefit of a new practice: ''Good program building provides for evaluation of results.''—Kelsey and Hearne.

STEP 6. COLLECT THE DATA. You should make every effort to get numerical data on the results of the test; only with such data will you be able to tell the farmers what amount of benefit they can expect to receive from the new practice on their own farms. Try to get actual measurements. Do not be content to take merely your impression of the results.

Although you may make a number of intermediate observations that are valuable, the ultimate information you want is the actual yield or other benefit of the practice. When you harvest the test plots, be sure that you also make a record of the size of each plot. Then you can convert the yields from plots of different sizes to yields from plots of the same size.

Here are some examples of data that have been collected in result tests:

Example 1. Data from part of a corn test.

Farm	New variety		Old variety		Yield per acre	
	Yield	Plot size	Yield	Plot size	New variety	Control
	(Bushels)	(Acres)	(Bushels)	(Acres)	(Bushels)	(Bushels)
Jones	9	1/4	100	5	36	20
Smith	28	1	25	1	28	25
Doe	16	1/2	1100	50	32	22

Example 2. Data from part of a feeding test on milk cows.

Farm	New feed		Old feed		Production per cow	
	Production	Number of cows	Production	Number of cows	New feed	Old feed
	(Pounds)		(Pounds)		(Pounds)	(Pounds)
Jones	89	4	653	36	22	18
Smith	627	20	576	20	31	29
Doe	148	10	211	19	15	11

Example 3. Data from part of a test of kudzu production.

Farm	Yield	Plot size	Yield per acre
			(Tons)
Jones	1.79 tons	10,000 sq. ft.	7.8
Smith	598 lbs.	1/10 acre	3.0
Doe	8584 lbs.	0.8 acre	5.4

STEP 7. INTERPRET THE RESULTS. The ultimate outcome of a result test is the issuance of a recommendation, a statement on how well the new practice works in the region. The data you have collected from the several test plots will now have to be interpreted for regional application. How valid this interpretation will be depends partly on how valid was the cross section of farms that was selected in the first place.

The interpretation of the results involves a number of statistical calculations, but fortunately none of these are difficult or complicated. Consider that much of the effort and planning you have expended thus far will have been wasted if you do not now bring the test to its proper conclusion with a proper statistical analysis of the data. In fact, much of the validity of the recommendations that ensue from the test will depend on your making this analysis.

Table 1 shows how to make an analysis for a test that is comparative. The data are from a test of a corn variety. First list the yields from the new and check practices for each location. The benefit is obtained by subtracting the check yield from the new-practice yield (note that farm 13 gave a minus benefit). Enter the squares of the benefits in the last column. Add all columns. The means, or averages, are obtained by dividing the sums by the number of farms.

Table 1.—Data from a corn variety test on 25 farms

Farm	Yield: New practice	Yield: Check	Benefit	Square of benefit
	Bushels per acre	Bushels per acre	Bushels per acre	
1. . . .	23	16	7	49
2. . . .	37	26	11	121
3. . . .	24	17	7	49
4. . . .	20	14	6	36
5. . . .	28	20	8	64
6. . . .	39	28	11	121
7. . . .	8	6	2	4
8. . . .	17	12	5	25
9. . . .	28	20	8	64
10 . . .	25	18	7	49
11 . . .	22	16	6	36
12 . . .	22	16	6	36
13 . . .	11	13	-2	4
14 . . .	21	15	6	36
15 . . .	18	14	4	16
16 . . .	17	12	5	25
17 . . .	37	26	11	121
18 . . .	31	22	9	81
19 . . .	14	10	4	16
20 . . .	21	15	6	36
21 . . .	28	20	8	64
22 . . .	24	17	7	49
23 . . .	22	16	6	36
24 . . .	28	21	7	49
25 . . .	26	19	7	49
Sum . .	591	429	162	1,236
Mean .	23.6	17.2	6.5	

Now you use the sum of the squares of the benefits, 1,236, to obtain a term known as the standard deviation (s.d.). The following calculations show how this is done:

Sum of squares of benefit	1,236
$\frac{(\text{Sum of benefit})^2}{\text{Number of farms}} = \frac{(162)^2}{25} = \frac{26,244}{25}$	1,050
Difference = 1236 - 1050	186
$\frac{\text{Difference}}{\text{Number of farms minus 1}} = \frac{186}{24}$	7.75
Standard deviation = square root of 7.75	2.8 bu.

$$\text{Standard deviation in percent} = \frac{\text{s.d. x 100}}{\text{check mean}} = \frac{2.8 \times 100}{17.2} = 16\%$$

You are now prepared to consider three pertinent questions in issuing recommendations to farmers.

Question 1. What was the average increase in yield from the practice?

Solution: Divide the mean benefit by the mean yield of the check and multiply by 100; i.e.,

$$\frac{6.5}{17.2} \times 100 = 38 \text{ percent}$$

Answer: Farmers on the average can expect an increase of 38 percent or 6.5 bushels per acre.

Question 2. What is the minimum increase in yield that farmers can expect 3 out of 4 times?

Solution: Multiply the standard deviation in percent by 0.7[3] and subtract the product from the average increase in percent; i.e.,

16 x 0.7 = 11.2 percent

38 - 11.2 = 27 percent

Answer: Three out of four times, the increase in yield will be at least 27 percent.

---

[3] The number 0.7 here is a mathematical constant used in statistics.

Question 3. What percent of the farmers are likely to get no increase in yield from the new practice?

Solution: Divide the mean benefit by the standard deviation to obtain a ratio, i.e.,

$$\frac{6.5}{2.8} = 2.3$$

Look up answer for this ratio in the following table, interpolating if necessary:

Ratio	Answer (Percent)
2.6	Fewer than $\frac{1}{2}$
2.3	1
2.0	2
1.6	5
1.3	10
1.0	15
.8	20
.7	25

Answer: About 1 percent of the farmers can normally expect to get no increase in yield from the new practice.

The statement you can make for farmers as a result of the test might be something like this:

> "What we consider a good cross section of farmers have participated in our test, and their results indicate that the new variety will increase yield in our region by 6.5 bushels per acre, or 38 percent, on the average; three-quarters of the farmers can expect at least a 27 percent increase; and fewer than 1 farmer out of 100 should fail to get an increase."

We might now illustrate the procedure for interpreting results when the test is not comparative—when you have data on a practice that does not compare with an old or check practice. In table 2 we have some data on the yield of kudzu. Interpreting these yield results is not so very different from interpreting comparative benefit results. Enter the squares of the yields in the last column and add both columns. The mean yield, 5.5 tons per acre, is the sum of the yields, 99.1, divided by the number of farms, 18.

Table 2.—Data for a kudzu test on 18 farms

Farm	Yield	Square of yields
	Tons per acre	
1	7.8	60.84
2	3.0	9.00
3	5.4	29.16
4	5.9	34.81
5	4.2	17.64
6	5.6	31.36
7	6.5	42.25
8	4.9	24.01
9	5.4	29.16
10	4.4	19.36
11	5.1	26.01
12	6.8	46.24
13	5.9	34.81
14	4.8	23.04
15	6.1	37.21
16	5.3	28.09
17	6.2	38.44
18	5.8	33.64
Sum	99.1	565.07
Mean	5.5	

The standard deviation is found in very much the same way as in the previous example:

Sum of squares of yield - - - - - - - - - - - - - - - - 565.07

$$\frac{(\text{Sum of yield})^2}{\text{Number of farms}} = \frac{(99.1)^2}{18} = \frac{9820.81}{18} \text{ - - - - - - - - - - } 545.60$$

Difference = 565.07 - 545.60 - - - - - - - - - - - - - - 19.47

$$\frac{\text{Difference}}{\text{Number of farms minus 1}} = \frac{19.47}{17} \text{ - - - - - - - - - - } 1.1453$$

Standard deviation = square root of 1.1453 - - - - - 1.07

$$\text{Standard deviation in percent} = \frac{\text{s.d.} \times 100}{\text{mean}} = \frac{1.07 \times 100}{5.5} = 19\%$$

With data that are not comparative, we might also consider three pertinent questions about the practice.

Question 1. What was the average yield of kudzu in the region?
Answer: 5.5 tons per acre.

Question 2. What is the least yield that three out of four farmers can expect?
Solution: Multiply the standard deviation by 0.7 and subtract the product from the mean yield; i.e.,

$$1.07 \times 0.7 = 0.749 \text{ tons per acre}$$
$$5.5 - 0.749 = 4.8 \text{ tons per acre}$$

Answer: Three times out of four, the yield will be at least 4.8 tons per acre.

Question 3. What is the minimum yield farmers can expect?
Solution: Multiply the standard deviation by 4[4/] and subtract the product from the mean yield; i.e.,

$$1.07 \times 4 = 4.28 \text{ tons per acre}$$
$$5.5 - 4.28 = 1.2 \text{ tons per acre}$$

Answer: It would be very unusual for the yield to be less than 1.2 tons per acre.

From these results, you would be justified in issuing a recommendation to the community along these lines:

> "Kudzu, when established on a representative cross section of farms in the region, yielded an average of 5.5 tons of forage per acre. While there is some variability over the region, three out of four farmers can expect at least 4.8 tons per acre; and practically all farmers should get 1.2 tons per acre as a minimum."

[4/] The 4 here is another mathematical constant.

A Guide to

extensive testing on farms

in four parts

PART III.

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a guide to

EXTENSIVE TESTING ON FARMS

by Henry Hopp

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Part III: Farm Experiments

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CONTENTS

INTRODUCTION

In Part I of this Guide we discussed the purposes and principles of extensive testing on farms. We said that when you are concerned with regional applicability of an agricultural practice, you need tests on farms as the next step after tests at the research station. Also, we distinguished two classes of extensive tests--result tests and farm experiments. The result test, which was discussed in Part II, helps the technician and the community learn how well an improved practice works under farm conditions. But it does not take care of all the needed tests of applicability on farms. For a result test involves just a single practice and has as its purpose the testing of the benefit of this practice alone.

Often, however, the technician has a more complicated problem: he does not know which of several alternative practices is the best under farm conditions. To find out, he must undertake an actual experiment on farms; and the way in which he can do so is the subject of Part III of this Guide.

Conducting farm experiments requires great care both in planning and in execution. In addition to the usual techniques of good experimental design, two requirements must be observed in designing farm experiments:

1. Keep the number of plots on any one farm to a minimum, so that no one farm is overburdened with plots.

2. Spread the test out over enough farms to make results representative of the region.

In order to show how the first of these requirements can be met, we give some designs in this part of the Guide that permit the plots per farm to be fewer than the number of practices under test. Most of these designs have been adapted from those that appear in the book Experimental Designs by Cochran and Cox 1/.

To help you design a farm experiment, we have outlined the procedure step by step. The first steps help you select an appropriate plan. Then an approximate but simple method is given for determining how many farms to have in the test. Following this, a procedure is shown for deciding on one plan when you have several to choose from. Finally, the field procedure itself is described.

Before you start using this part of the Guide, be sure to read Part I, which presents the principles of extensive testing.

1/ William G. Cochran and Gertrude M. Cox. Experimental Designs. John Wiley and Sons, Inc., New York, 1950.

FARM EXPERIMENTS OR RESEARCH-STATION EXPERIMENTS?

An experiment on farms is likely to be more expensive than an experiment at the research station, even though the cost may be partly reduced by spreading the work out among more people. Besides, an experiment is more inconvenient to operate on several dispersed farms than at a single research station. Hence, before starting an experiment on farms, you should be sure that this approach is what the problem really requires.

You might keep this simple rule in mind as a starting point for deciding which type of experiment is required:

> When you do not know whether the practices under test are effective, conduct the experiment at a research station (or on just one or two experimental sites); when you are reasonably sure that the practices are effective but do not know their regional applicability, conduct the experiment on farms.

A more detailed presentation of the idea of regional applicability is given in Part I of this Guide.

WHAT IS MEANT BY DESIGN?

Let us consider what is meant by "design." What are you trying to accomplish? After all, you might ask, what else do we need to do besides trying out the treatments on each farm?

In some cases, that is all there is to the problem of design: merely a testing of different practices on several farms. More often than not, however, such a simple procedure is not practical. For example, what if you want to test 8 treatments? Considering the difficulty of conducting cooperative tests on farms, you may not want this many plots on one farm. You can overcome the difficulty by selecting a plan that, for instance, permits you to test 8 treatments and yet have no more than two plots on any one farm. In the Appendix, beginning on page 31, are various plans that you can use when you want to test more treatments than you can put on a single farm.

Biometricians have developed plans also that will help increase the precision of the test. By using these plans you can reduce the number of replications, or repetitions, that are required and therefore reduce the cost of the extensive test.

But design means more than selecting a plan. It also means determining the number of farms required for the test, and the number of replications that should be used. For you must have replication of farms if you are to get a representation of farms and a measure of the consistency of the results over a region.

SELECTING A PLAN

Most of the plans shown here are for testing 10 treatments or less. Those calling for a larger number are used in testing varieties of a crop, or testing factors in different combinations and at different levels--such as nitrogen, phosphorus and potassium in different combinations and amounts. Most of the plans are confined also to just a few plots--four or fewer--on any one farm. All the plans are to be followed without repetition in any one location.

If you desire to have more treatments or more plots per farm than in the plans included in this Guide, you will have to refer to a more exhaustive source of information on experimental design, such as the book by Cochran and Cox[2/]. However, before you do that, it might be well to reconsider your objectives, recognizing that extensive tests are intended to determine the applicability of practices that are already known to be effective.

The practices, in the first place, should be fairly well screened to eliminate those that are obviously not worthwhile. If you have a large number of practices to test, possibly the screening has not been adequate. It would be better, then, not to conduct an extensive test, but rather to begin with a screening experiment at one or two localities, probably at a research station, using a conventional research design. This research test will permit you to eliminate those treatments that are likely to be ineffective, impractical, or definitely inferior. Thereafter you will be ready to conduct the extensive test, using only practices that are likely to be effective and for which, in the region under consideration, you need the information on relative applicability.

All the plans shown call for only a few plots on each farm. It is generally inadvisable to have a large number of plots on any one farm because there is too much danger that the technician and the cooperating farmer will get the plots confused. If you are thinking of a large number of plots on each farm, reconsider the idea. Avoid getting involved in a complicated study when you are making cooperative tests.

There is a further disadvantage in having a large number of plots on any one farm: it will tend to concentrate the test at too few locations. Since the principal purpose of an extensive test is to get a representative answer for a region, it is better to have a few plots per farm and more farms than to have many plots on only a few farms.

2/ *Op. cit.*

STEP 1
DECIDE ON THE NUMBER OF TREATMENTS

The treatments are the different practices you are testing: treatments applied to the land or crop, or different varieties of a single crop. Often the purpose is to compare improved practices with a current practice. The current practice is called the check, or control, and it counts as one of the treatments. Thus, if you want to test 4 new varieties of wheat in comparison with the native variety of wheat, you will have a total of 5 treatments. Sometimes the check is not actually the same practice on every farm: the native wheat, for instance, may comprise as many different varieties as there are farms. But having exactly the same variety as a check on every farm is not necessary, for the purpose of the test is to compare new practices with a current practice, whatever that may be.

STEP 2
CLASSIFY THE EXPERIMENT

Table 1 lists the various plans from which you will select the one best suited to your testing problem. To facilitate your choice, first classify the experiment into one of three types:

Type I

Experiments are put in Type I when they involve 10 or fewer treatments; treatments should not be combinations of factors at several levels. For example, you may be comparing several different insecticides. Each insecticide is a treatment, and the check is an additional treatment. The purpose of the experiment, then, is to compare the several different insecticides with the check, and determine which insecticide gives the best results; and the experiment is simply a comparison among several separate items.

Type II

Experiments are put in Type II when they involve more than 10 treatments; as in Type I, they should not be combinations of factors at several levels. Such tests apply most often to varieties of a crop. As a rule you will not want to test a very large number of varieties in an extensive test. You can usually eliminate the unadapted varieties by a

Table 1. Key to selecting plan for an extensive test.

TYPE I
10 treatments or less

Number of treatments	Number of plots on each farm	Plan to use
2	1	A
	2	B
3	1	A
	2	C D-1
	3	B
4	1	A
	2	C D-2
	3	E-1
	4	B
5	1	A
	2	C D-3
	3	E-2
	4	F-1
6	1	A
	2	C D-4
	3	E-3
	4	F-2
7	1	A
	2	C D-5
	3	E-4
	4	F-3
8	1	A
	2	C D-6
	4	F-4
9	1	A
	2	C D-7
	3	E-5
	4	F-5
10	1	A
	2	C D-8
	3	E-6
	4	F-6

TYPE II
More than 10 treatments

Number of treatments	Number of plots on each farm	Plan to use
16	4	G-1
25	5	G-2
27	3	G-3
36	6	G-4

TYPE III
Combination of factors at several levels

Number of Factors	Number of Levels	Number of plots on each farm	Plan to use
2	2	2	H-1
		4	H-2
	3	3	H-3
		9	H-4
	4	4	H-5
3	2	4	H-6
		8	H-7
	3	9	H-8
4	2	4	H-9
		8	H-10

research test at the experiment station. Occasionally, however, you may have more than 10 varieties to test. With such a large number of varieties, you will test only a few of them at any one location so as not to have too many plots on each farm.

Type III

Experiments are put in Type III when the treatments are combinations of factors at several levels. An example is a fertilizer test in which the effects of nitrogen, phosphorus, and potash are being determined alone and in various combinations. The factors are the individual fertilizer elements; levels are the amounts or rates at which they are used.

When a fertilizer element is used at two rates, such as present and absent, or high and low, we say that the factor is at two levels. If we go on to testing two different rates of a fertilizer in addition to none, we say that the factor is at three levels. It is not often advisable to test more than three levels (plans given here are for tests up to 4 factors at 2 levels). If you test the minimum, average, and maximum amounts of fertilizer that are practical, you will be able to draw a curve of the response, and thereby get a reasonably good idea of the most economic level.

Although determining the optimum economic level of various fertilizer combinations is a common problem in extensive testing, experiments in Type III need not be confined to fertilizers. One of the factors might be a practice, such as applying green manure or an insecticide.

Each of the Type III plans referred to in Table 1 tests an equal number of levels for all factors. Some factors are tested at two levels; some, at three; one, at four. If you want to test an unequal number of levels in the same experiment, you need more complicated designs: consult chapters 5 and 6 of the book by Cochran and Cox[3].

STEP 3
DECIDE ON THE NUMBER OF PLOTS ON EACH FARM

Generally you will want to have only a few plots at each farm. In deciding on the number, consider the time available to each field technician involved and the land available on the cooperating farms. Plans are shown for as few as one and two plots per farm.

3/ Op. cit.

The number of plots per farm is limited somewhat by the number and type of treatments. For Type I tests, plans are given here that permit you to have up to 4 plots on a farm with almost any number of treatments from 2 to 10.

STEP 4
SELECT THE APPROPRIATE PLAN

You are now ready to consult Table 1 for selecting the appropriate plan. In the column under "Plan to use" you will see 8 series, A to H. General characteristics of each series are as follows:

- A. Each treatment is on a different farm, with only 1 plot per farm.
- B. All treatments are on each farm.
- C. Each treatment is on a different farm, as in series A; but each farm also has a check plot, making 2 plots per farm.
- D. Two treatments are on each farm, but there are 3 or more treatments in all.
- E. Three treatments are on each farm, but there are 4 or more treatments in all.
- F. Four treatments are on each farm, but there are 5 or more treatments in all.
- G. There is a large number of treatments or varieties (16 up to 36).
- H. Treatments are factorial; i.e., two or more factors are applied in combination at different levels.

Suppose you want to test 3 separate treatments and have decided on 1 plot for each farm: these specifications will lead you, in the third column of table 1, to select Plan A. For detailed layout of a single repetition of that plan, see page 31. Or maybe you want to test 4 separate treatments and have decided on 3 plots for each farm. Such specifications call for plan E-1; layout is shown on page 42.

Occasionally you will find you have a choice of plans. For example, with 4 treatments and 2 plots at each farm, there are two possibilities: Plans C and D-2. We will have more to say on pages 17-20 on how to choose among plans when there are several possibilities.

Suppose your test involves a large number of separate treatments and so falls in Type II. Plans given here for this type of test cover numbers of treatments between 16 and 36. If the number of treatments you want

is not included in the list, you can follow one of two alternatives. The first is to repeat several of the treatments to round out the total number to fit into one of the designs shown. For example, if you have 23 varieties to test, you might repeat two, for a total of 25, and then use plan G-2. The second alternative is to refer to Cochran and Cox's book on experimental designs,[4/] where more plans are shown. Most of these plans, however, are more complicated.

But suppose your test involves combinations of factors at different levels and so falls in Type III. Here the specifications become a bit more complex: instead of just the number of treatments, you must specify both the number of factors under test and the number of levels.

For example, if you are testing nitrogen, phosphorus, and potash, you have three factors; and if you are testing just the absence and presence of each element, you have two levels. With 3 factors at 2 levels, you will have 8 possible treatment combinations ("1" means absent or the lower level; "2" means present or the higher level):

1. $N_1P_1K_1$	5. $N_1P_1K_2$
2. $N_2P_1K_1$	6. $N_2P_1K_2$
3. $N_1P_2K_1$	7. $N_1P_2K_2$
4. $N_2P_2K_1$	8. $N_2P_2K_2$

There are one or more possible plans for each combination of factors and levels, according to the number of plots on a farm. With 3 factors at 2 levels, there is a plan for 4 plots per farm (H-6, on page 63) and another for 8 plots per farm (H-7, on page 64). If it is immaterial whether you have 4 or 8 plots per farm, you might refer to Step 5 of the next section (pages 17-20) for guidance.

If you want to find plans for a larger number of factors or for factors at different levels, you can find suggestions in the book by Cochran and Cox[5/]. However, you should be wary of tests that involve a large number of variables and complicated designs. In extensive testing it should be possible to answer most problems with relatively simple designs.

4/ _Op. cit.,_ Table 9.5

5/ _Op. cit.,_ Table 6.19

DECIDING ON THE NUMBER OF FARMS

The four steps you have just taken have led you to one or more possible plans for the experiment. You now have to answer the question: How many farms should be included in the experiment? That is, how many repetitions of a plan are required to get an adequate test of the region? By following the first four steps in this section, you will be able to determine the number of farms and repetitions that are required. If you have several possible plans, you must choose among them; the fifth step tells you how to do that.

STEP 1
ESTIMATE THE MINIMUM DIFFERENCE

The decision to be made now is this: How small an improvement can a treatment give and yet prove worthwhile; in other words, what is the minimum difference youwant to test? At first this may appear to be a difficult decision, but it is in fact quite simple for any experiment that has a practical objective.

It is easy to see why this decision must be made: the smaller the effect, the more repetitions are needed. If you are dealing with a practice that increases yield to a spectacular extent, you can get by with a rather small extensive test. But if the practice produces only a small effect, you will need a much more thorough test. Fortunately, most extensive tests involve practices that give large benefits or large increases in yield.

One of two methods can be used to determine the minimum difference to be tested: One is based on the cost of applying the practice; the other, on the effort required to put the practice into effect.

On the basis of cost

Let us say that you are undertaking an extensive test to determine whether it is beneficial to apply fertilizer and have selected an application rate of 300 pounds to the acre. If fertilizer costs $55 a ton, the cost of the practice would be approximately $8 per acre for the fertilizer and perhaps $2 more for applying it, or a total of $10 per acre. If the crop under test is corn, worth $2 a bushel, the farmer will have to get an increased yield of 5 bushels per acre to pay the cost of the practice. If the average yield of corn without fertilizer is 30 bushels to the acre, the minimum increase needed would be 1/6, or approximately 16 percent. For the farmer to show a profit from the practice, an even larger increase in yield would be needed. Let us say, then, that you set the minimum difference at something above 16 percent, perhaps 20 percent.

A similar method is used when you are testing factors at several levels, that is, making Type III tests. Continuing the illustration, suppose that you wish to test 3 levels of fertilizer--0, 150, and 300 pounds to the acre. Each level is 150 pounds higher than the preceding one; and 150 pounds, with the labor of applying it, costs, let us say, $5. Going through the same computations as in the previous paragraph, you find that an increase of 8 percent in yield would pay this cost; a little more than 8 percent, then, is the minimum difference you will be interested in testing.

<u>On the basis of effort required</u>

Sometimes the method based on cost is impractical or gives unrealistic results and must be tempered by judgment. The minimum increase that would be profitable by the cost method--say 5-percent for a certain practice--might be too small to compensate for the effort that farmers would have to put forth to make the change. You might then decide that unless the practice results in, say, a 20 percent benefit, it has no promising basis for an extension program.

Thus, you see, you may decide on the minimum difference quite subjectively. The decision may even involve program policy. Several alternative extensive tests may be proposed; yet your organization may have facilities for incorporating only a few of the practices into its technical program. If so, practices that give only a small benefit will not be of much interest. Of course, if the practice is not likely to produce the minimum difference, you should not consider it further.

STEP 2
ESTIMATE THE ERROR

When you try out a practice at different places, you do not always get the same result. Sometimes the practice is good, sometimes not. This random, accidental, or unexplained variability in different places is called the error of the test. After the test is completed, you will find the actual error and you will use it to set up confidence limits for recommendations to farmers. But before you start the test, you have to make an "informed guess"--or estimate--of what the error will be. You need to do so because the greater the error, the more repetitions you will need for precision.

A simple, approximate method will be described here to estimate the error. It is based on your knowledge of the area and the practice being tested. There are more advanced methods, in which you use actual data from previous surveys or research tests and make statistical computations on a calculating machine. These methods are more

exact. If you do not feel satisfied with the method given in this chapter, you might want to use the more exact methods. These are given in Part IV of this Guide.

To determine the error of the test, you must make some guesses on the three factors of variability:

1. Plot variability: How much do adjacent plots on the same farm vary in yield?

2. Location variability: How much do farms in the same region vary in yield?

3. Treatment variability: How much does the benefit from a new treatment or practice vary from farm to farm?

If you intend to use Plan A, you will need to guess at all three of these. But for Plans B to H, you can skip the second--location variability.

Estimating plot variability

Consider two plots of the crop in question. They are side by side and are treated the same way. Sometimes the two plots will actually have the same yield, but more often they will not. Now answer the question, "What is the maximum difference in yield that we might obtain between these two plots?" Note that we want the maximum difference, not the usual difference.

You see that the decision does not require actual data; it is reached by guess. It might be wise to discuss your guess with several of your associates; perhaps they can even help you make it.

As an example, let us say that the test is with 1/4-acre corn plots. Further, let us say that the average yield of corn in the area is 30 bushels per acre. The question, then, is this: "If two plots, side by side and treated alike, gave this average yield, what would be the extreme, or maximum, difference that we might anticipate between them?" You now answer this question, perhaps after consulting with several competent people. You might conclude that two 1/4-acre plots side by side, with an average yield of 30 bushels per acre, might, at the extreme, have a difference of 20 bushels per acre, one plot yielding 40 bushels and the other 20. This is your estimate of the maximum difference.

Having arrived at a maximum difference, you obtain the value for plot variability by dividing the maximum difference by 6:[6]

$$\text{Plot variability} = \frac{\text{Maximum difference}}{6}$$

$$= \frac{20}{6}$$

$$= 3\text{-}1/3 \text{ bushels}$$

A slightly different way of making the same guess is by thinking of the average, rather than the maximum, difference. You might try this procedure as a check on the reasonableness of the first procedure. Now you ask the question this way: "If we take the yields on two adjacent plots, what on the average will be the difference between them, when the two plots together have a mean yield of 30 bushels per acre?"

Clearly, two adjacent plots might have almost the same yield or they might be quite divergent. But, after considering the question, and discussing it with your associates, you may decide that, on the average, two adjacent plots might differ by 5 bushels. The plot variability is now obtained by dividing this difference by the number 1.4:[7]

$$\text{Plot variability} = \frac{\text{Average difference}}{1.4}$$

$$= \frac{5}{1.4}$$

$$= 3.6 \text{ bushels}$$

Using the two procedures, you might decide on a plot variability of 3.5 bushels. This value is now expressed as a percent of the mean yield, which is 30 bushels per acre:

$$\text{Plot variability} = \frac{3.5 \times 100}{30}$$

$$= 12\%$$

Estimating location variability

Skip this section if you are considering plans B to H. You need determine location variability only for Plan A tests.

Location variability is merely the variability in yield between farms that are handled by usual farming methods, that is, the differences in yield that farmers in the region obtain. Location variability combines differences due to climate, soil fertility, and farm practice.

6/ George W. Snedecor, Statistical Methods, 4th ed., 1946, p. 98.
7/ Ibid., p. 49.

To determine location variability, you use a method very much like the one for plot variability: you draw upon your own experience and upon the judgment of other competent technicians or farmers in the area. Answer this question: "For the crop and for the region in which the extensive test will be conducted, what is the highest and lowest yield per acre that farmers obtain?" Note, again, that we want the maximum difference.

As an example, let us say that the test will be conducted on corn; that your estimate of the highest and lowest yields is 100 and 10 bushels per acre, respectively; and that the mean yield is 30 bushels per acre. The location variability is obtained simply by dividing this maximum difference by 6:[8/]

$$\text{Location variability} = \frac{\text{Maximum difference}}{6}$$

$$= \frac{100 - 10}{6}$$

$$= 15 \text{ bushels}$$

Now convert this value to percent simply by multiplying by 100 and dividing by the mean:

$$\text{Location variability} = \frac{15 \times 100}{30}$$

$$= 50\%$$

Estimating treatment variability

In some places a given practice will certainly work better than in others. Sometimes it will produce a very large effect, sometimes little or perhaps none. Now, answer this question: "Considering that a treatment will not always have the same effect, what will be the very most and the very least effects that we can expect?"

If the treatment produced exactly the same increase in yield everywhere it was applied, the treatment variability would be zero. Such a result, however, is uncommon. More often, results will be somewhat inconsistent among the different locations. The decision you must now make is this: What is the probable inconsistency of the results?

You doubtless have a pretty good idea what the average effect of the treatment will be; let us say that you anticipate an average increase in yield of 50 percent. Of course there will be some variation; not all locations can be expected to show this much improvement. In some

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8/ Snedecor, op. cit., p. 98

places, the treatment may work well, in others, not so well.

You may be quite certain, for example, that in no place will you get more than 100 percent increase from the treatment. This, then, will be the upper limit of the treatment effect. The lower limit will be the least effect that you anticipate--perhaps a 20-percent increase. Or you may expect that in some cases you will get no increase in yield at all. Then the lowest treatment effect would be zero.

By this means, you arrive at two extreme values: the highest and lowest yield increases that you can anticipate from the treatment. The difference between these two extreme values, divided by 6, gives the treatment variability:

$$\text{Treatment variability} = \frac{\text{Maximum difference}}{6}$$

$$= \frac{100 - 0}{6}$$

$$= 17\%$$

Another way of making the same estimate is in actual measures rather than in percent. You start by making estimates like those shown in the following example:

Average yield of check plot - - - - - - - - - - - - -	30 bu. per acre
Average increase in yield due to treatment - - - - -	20 bu. per acre
Maximum increase in yield due to treatment - - - - -	30 bu. per acre
Minimum increase in yield due to treatment - - - - -	0 bu. per acre

Then the treatment variability is computed as follows:

$$\text{Treatment variability} = \frac{\text{Maximum yield - minimum yield}}{6}$$

$$= \frac{60 - 30}{6}$$

$$= 5 \text{ bushels}$$

This value is multiplied by 100 and divided by the mean yield without treatment to get the treatment variability in percent:

$$\text{Treatment variability} = \frac{5 \times 100}{30}$$

$$= 17\%$$

When you are testing several treatments in one extensive test, make the estimate of treatment variability for the treatment or treatments you were thinking of when you estimated the minimum difference (page 9).

Combining the estimates of variability

Once you have the estimates of variability, putting them together to get an error for the extensive test is just a matter of simple arithmetic. You merely square the variabilities, add the squares, and then take the square root of this sum. For Plan A, you use all three variabilities:

1. Plot variability = 12%; squared = ~~400~~ 144
2. Location variability = 50%; squared = 2500
3. Treatment variability = 17%; squared = 289

Total of the squares 2933
Extensive-test error (square root of 2933) - - - 54%

For Plans B to H, you use just the plot and treatment variabilities;

1. Plot variability = 12%; squared = 144
2. Treatment variability = 17%; squared = 289

Total of the squares 433
Extensive-test error (square root of 433) - - - 21%

STEP 3
DETERMINE THE NUMBER OF REPETITIONS

After you have selected a plan and have estimated the minimum difference and the error, you can determine the number of repetitions of the plan by simple arithmetic. You can see how to do it by an example.

Let us say that you have chosen Plan E-2 (page 43); that is, you are testing 5 treatments with 3 plots per farm. Also, you have made the following estimates:

Minimum difference to be tested - - - - - 30%
Error - - - - - - - - - - - - - - - - - - 21%

Now divide the difference by the error:

$$\frac{30}{21} = 1.43$$

Then, in table 2,[9] find 1.43 or the numbers closest to it in the first column (1.25 and 1.50). The number of replications required

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9/ Cochran and Cox, op. cit., Sec. 2.21

Table 2. Number of replications required for extensive tests on the basis of the ratio between minimum difference and error.

Ratio: difference divided by error	Number of replications
10	3
7.5	3
5	4
4	4
3.5	4
3.0	5
2.5	6
2.0	8
1.75	9
1.50	12
1.25	16
1.00	23
.90	28
.80	35
.70	45
.60	60
.50	84

is estimated in the other column. About 12 replications are required for the whole test.

Finally, look at the description at the head of Plan E-2 (page 43) to find that it has 6 replications for each repetition. Therefore--

$$\text{Times to repeat the plan} = \frac{\text{Number of replications required for test}}{\text{Number of replications in each repetition}}$$

$$= \frac{12}{6}$$

$$= 2$$

Now you know that you will need to repeat Plan E-2 twice.

If you look at Plan G-1, you will note that it, like some others in the Appendix, provides several arrangements for each repetition. If you have to repeat the plan, use as many of the arrangements as you can. Thus, if 2 repetitions are required, use arrangements 1 and 2 once each, rather than arrangement 1 twice.

STEP 4
DETERMINE THE NUMBER OF FARMS

Suppose again that you are using Plan E-2 and have determined that you need 2 repetitions. As is shown on page 43, 1 repetition calls for 10 farms; therefore you will need 20 farms in all.

STEP 5
CHOOSE AMONG SEVERAL PLANS

Now you come to the matter of choosing when Table 1 offers more than one plan for a given number of treatments. For example, you may wish to have 7 treatments but are undecided as to the number of plots per farm. Table 1 offers you 5 possible plans for 7 treatments: A, 1 plot per farm; C, 2 plots; D-5, also 2 plots; E-4, 3 plots; and F-3, 4 plots.

Which of these designs should you use? Begin by summarizing in tabular form, Steps 1 to 4 of this section for the several possible designs. Table 3 is such a summary for the example under consideration.

Table 3. Example of a summary to aid in choosing among several possible plans.

SPECIFICATIONS

No. of treatments: 7 (1 is a check)
No. of plots at each location: 1, 2, 3, or 4
Minimum difference: 30%
Anticipated error, Plan A: 54%
Plan B to H: 21%

(1) Possible plans (Table 1)	(2) Plots on each farm (Table 1)	(3) Difference ÷ Error	(4) Replications required (Table 2)	(5) Replications per repetition (Appendix)	(6) Repetitions required (Cols. 4 ÷ 5)	(7) Farms per repetition (Appendix)	(8) Plots per repetition (Appendix)	(9) Total Requirement: Farms (Cols. 6 x 7)	(10) Total Requirement: Plots (Cols. 6 x 8)
A	1	0.56	68	1	68	7	7	476	476
C	2	1.43	12	1	12	6	12	72	144
D-5	2	1.43	12	6	2	21	42	42	84
E-4	3	1.43	12	3	4	7	21	28	84
F-3	4	1.43	12	4	3	7	28	21	84

At the head of the table are listed the specifications for the design: 7 treatments and 1, 2, 3, or 4 plots per farm. The minimum difference to be tested has been determined, by Step 1 of this section, as 30 percent. The error, as determined by Step 2, is 54 percent for Plan A and 21 percent for Plans B to H.

You are now ready to fill out the 10 columns of the table. In column 1 copy the possible plan numbers offered in table 1 and in column 2 list the number of plots per farm that each plan calls for. In column 3 list the ratio of difference to error: For Plan A, it is 30/54, or 0.56; for the other plans it is 30/21, or 1.43. In column 4 give the number of replications that are required; this you will get from table 2, which indicates 68 replications for a difference-error ratio of 0.56, and 12 for a ratio of 1.43. In column 5 list the number of replications in each plan. In column 6 give the number of repetitions required--a value you will arrive at by dividing the number in column 4 by its corresponding number in column 5. Columns 7 and 8 you will get from the appendix. Column 9, which gives the total number of farms required for the extensive test, is obtained by multiplying column 6 by column 7. Column 10, the total number of plots in the test, is obtained by multiplying column 6 by column 8.

The last two columns contain the information that you need for making a choice among the possible designs. Note that the number of farms (column 9) decreases as the number of plots per farm (column 2) increases. With 1 plot per farm (Plan A) you will need 476 farms; with 4 plots per farm (Plan F-3) you will need only 21.

Obviously, it is better to have a few plots per farm, provided the required number of farms is not too great. But, in this example, Plan A calls for an excessive number of farms. Almost surely you cannot operate an extensive test that requires 476 farms; therefore you will eliminate Plan A.

So you move on to Plan C, which has 2 plots per farm. It requires 72 farms, still a fairly large number. Plan D-5 also has 2 plots per location, but it requires only 42 farms.

The one advantage of Plan C, however, is that it has both a treatment and a control on each farm. This fact may give the test better demonstration value, for the farmer will be able to see the effect of the new practice more easily. However, to get this added demonstrational value, you have to use 72 locations instead of 42, or approximately 75 percent more locations.

If both these plans require too many locations, you might go on to Plan E-4, which calls for 3 plots per farm but only 28 farms. Finally, Plan F-3, with 4 plots per farm, reduces the number of locations to 21. Plans E-4 and F-3 require the same number of total plots, 84, and you

choose between them on the basis of your preference for fewer farms or for fewer plots per farm. You might decide, for example, that 4 plots are too many for each location, but that 3 would be satisfactory. Then you choose Plan E-3. Or if you think the additional 7 farms will be a serious handicap, you will decide in favor of Plan E-4.

FIELD PROCEDURE

STEP 1
SELECT THE REGIONS AND THE FARMS

In laying out an extensive test, you will first have to decide whether results are desired separately by regions within the area where the practice is to be applied. The point is discussed in Part II of this Guide, where you will find help in making the decision. If you decide to get results separately by regions, lay out a complete design in each region.

With more than one region to test, you may find that your estimates of the minimum difference or of the error may differ among regions. If so, determine the required number of farms separately for each region.

Methods of selecting the farms and locating the plots on each farm also have been discussed in Part II.

STEP 2
ASSIGN THE BLOCKS TO THE FARMS

Note that all plans shown in the Appendix are drawn up in blocks, with one plot or more in each block. Each block goes on a different farm. Your next job is to assign the blocks to the farms.

In each plan the blocks are numbered consecutively. You will have to rearrange the blocks; for, if you assign them in the order shown, each treatment will tend to be concentrated in one section of the region and the effects will not be representative for the entire region.

There are two schemes for assigning the blocks to the farms. Scheme 1[10/] is to subdivide the region and put a separate repetition in each sub-

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10/ Scheme 1 permits measurement of subdivision variability. Selection of plans suitable for this purpose is made somewhat arbitrarily. In general, it has been drawn at 20 degrees of freedom for error when there are two repetitions. Table 6 shows that the degrees of freedom for error depend on the number of repetitions. But some plans will have a large number of degrees of freedom for error, even with just 2 repetitions; with such plans we have an opportunity to get more information without extra work. However, for the plans that are not likely to have 20 degrees of freedom in the error, such a subdivision of the region would not be advisable and Scheme 2 is indicated.

division. You can use this scheme only if you need repetitions, and then only with the following plans:

D-4 to D-8
E-2, E-3, E-5, E-6
F-2 to F-6
G-1 to G-4
H-5, H-8 and H-9

For the other plans--and for all plans when they are not repeated--use scheme 2.

Scheme 1.

To show you how to fit a plan that permits subdivision of the region, we will use Plan G-1 (page 54) as an example. This plan calls for 4 blocks per repetition and gives 5 possible arrangements, each one of which counts as a repetition.

Let us say you have determined by the procedure given on page 17 to use 4 arrangements repeated twice. This calls for a total of 32 farms. The arrangements and repetitions are assigned to different subdivisions of the region, but each set of 4 blocks must be in the same subdivision. Hence, you will have 8 subdivisions and 4 farms per subdivision. Figure 1 shows how the subdividing may be done; here it is partly physiographic, partly political, and partly by extension districts.

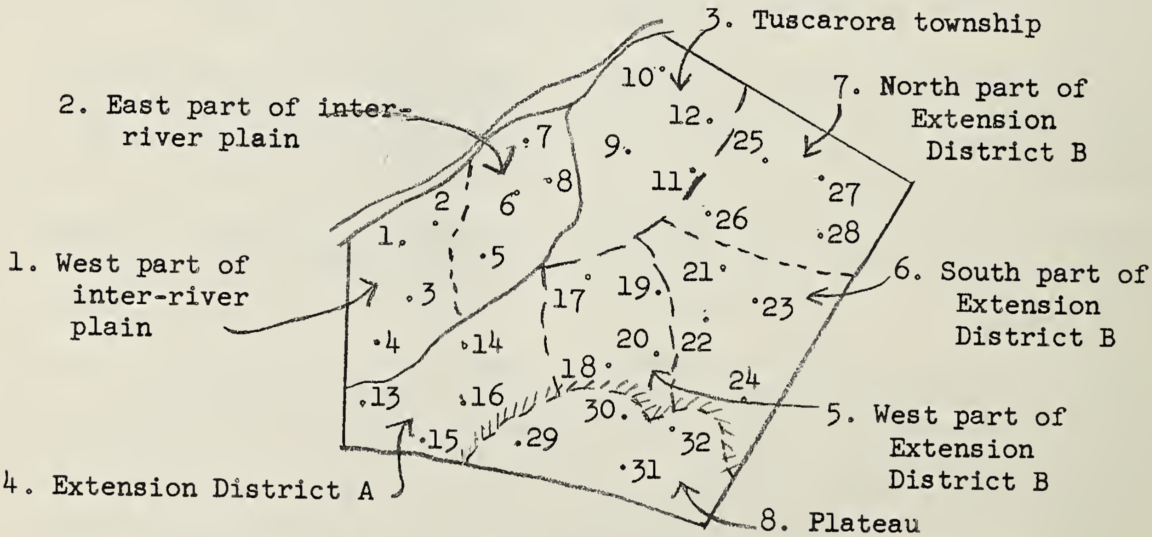

Figure 1. Numbering the farms for Plan G-1 in a test region that has been partitioned into subdivisions.

The rest of the procedure is shown in Table 4. The columns for subdivisions, farms, and arrangements (each repeated twice) are listed in consecutive order. But within each subdivision the blocks are assigned at random--"out of the hat." For plans that do not have a choice of arrangements, substitute a repetition column for the arrangement column.

Scheme 2.

When you are using one of the plans for which subdivision is not advisable, or when you are not repeating a plan, follow the scheme shown in Figure 2. To sharpen the contrast between the two schemes, Figure 2 has been made for the same area as Figure 1. Now, though, you pay no attention to subdivisions. The farms are numbered consecutively over the region. As you might expect, the numbering order corresponds approximately to the geographic order. This time we are fitting Plan E-1 (page 42) to the region. It calls for 3 plots at each farm and requires 4 farms for a single repetition. Let us say that you have determined to use 8 repetitions, or a total of 32 farms. The assignment of treatments to the farms is shown in Table 5; you list the blocks and repetitions in consecutive order, but list the farms in randomized order.

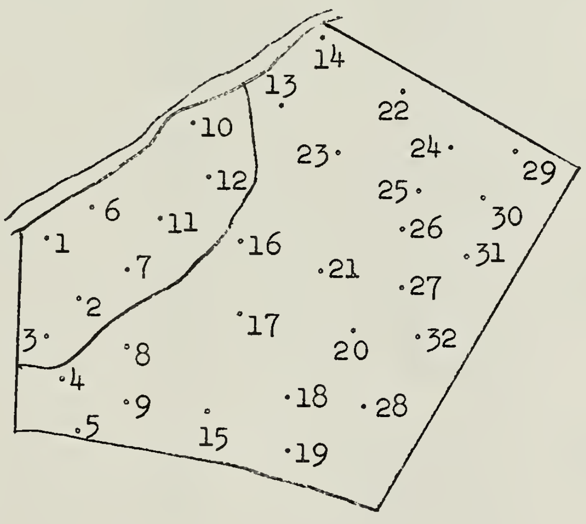

Figure 2. Numbering the farms for Plan E-1 in a test region that has not been partitioned into subdivisions.

Table 4. A convenient scheme for fitting Plan G-1 (4 blocks per arrangement) when 4 arrangements are repeated twice and the 32 farms are numbered in approximate geographic order in the test region.

Subdivision	Farm	Arrangement	Blocks (randomized)
1. Inter-river plain (west)	1	1	3
	2	1	1
	3	1	4
	4	1	2
2. Inter-river plain (east)	5	2	4
	6	2	1
	7	2	2
	8	2	3
3. Tuscarora township	9	3	4
	10	3	1
	11	3	3
	12	3	2
4. Extension District A	13	4	1
	14	4	3
	15	4	4
	16	4	2
5. Extension District B (west)	17	1	2
	18	1	3
	19	1	4
	20	1	1
6. Extension District B (south)	21	2	4
	22	2	2
	23	2	1
	24	2	3
7. Extension District B (north)	25	3	3
	26	3	1
	27	3	4
	28	3	2
8. Plateau	29	4	3
	30	4	1
	31	4	2
	32	4	4

Table 5. A convenient scheme for fitting Plan E-1 (4 blocks per repetition) when there are 8 repetitions and the 32 farms are numbered in approximate geographic order in the test region.

Block	Repetition	Farm (randomized)
1	1	3
2	1	16
3	1	12
4	1	18
1	2	26
2	2	23
3	2	31
4	2	27
1	3	29
2	3	11
3	3	14
4	3	20
1	4	5
2	4	7
3	4	17
4	4	9
1	5	24
2	5	22
3	5	1
4	5	21
1	6	32
2	6	28
3	6	19
4	6	8
1	7	15
2	7	4
3	7	13
4	7	30
1	8	25
2	8	10
3	8	6
4	8	2

Once the blocks are assigned to the farms you will be able to tell from the plan just what treatments go on the plots of each farm. Your next job is to lay out the plots and assign the treatments to them. In selecting the plots, one would like to select areas that are exactly alike in all respects, but it is impossible to do so. No set of plots will ever be found exactly alike. There are sure to be some differences in productivity among them. If you attempt to select plots that are of equal fertility, you are up against the same problem you face in attempting to select "typical farms."

The safest as well as the simplest procedure is merely to assign the treatments at random to the plots. This assures that the effects of a treatment will be fully representative of the results farmers will obtain when they use the practice at random in their fields.

You must be careful not to bias your results. If a new practice is tested on land that has been selected because it is more fertile than the land on which the old practice is used, the result of the test will be biased and may be very much in favor of the new practice. It will not be a valid measure of the true effect of the practice. Rather, the effect will be confused, or, as the biometrician says, "confounded," with the natural fertility differences between the two selected plots of land. Such selection may be considered a good idea for the purposes of demonstration. But if the purpose of the work is to find out the true effect of the practice so that you can be correctly guided in your recommendations to farmers, it is essential that the treatment effects should not be biased by natural differences in soil fertility. If you lay out the plots and then simply assign the treatments at random to the plots, you will assure against this bias.

STEP 3
COLLECT THE DATA

The data for each plot are obtained separately. At times you will feel that the yields are unrepresentative, especially when the new practice fails to yield as well as the check practice. But be slow to eliminate or "correct" plot yields solely because they are not in line with expectations. Just remember that some variability is to be expected; in fact, one purpose of the extensive test is to get a true idea of variability in the effectiveness of the practice. Eliminate a plot only if you are certain that something was done wrong during the test. Even if some of the plots have to be eliminated, or are missing, a proper analysis can still be made.

STEP 4
ANALYZE THE DATA

Analyze the data by the usual statistical methods. The analysis will eliminate farm differences so that you can estimate the true, unbiased effects of the treatments. The analysis also gives the confidence limits, that is, tells how much the benefit from each treatment may be expected to vary. Conclude your analysis with some statement like this:

> As a result of the test we recommend variety A for this region. On the average, an increase in yield of 50 percent can be expected. Three-fourths of the time, the increase will be at least 37 percent. Only about 1 farmer out of every 100 can expect no increase in yield.

Such an objective, straightforward, and conclusive statement can be made only if you have completed a statistical analysis. The methods need not be repeated here; they are fully described in several books on statistical methods. For your convenience, however, we are referring only to the book by Cochran and Cox.[11/] Table 6 will be useful to the technician who does the statistical analysis. The degrees of freedom to use in the analysis of variance, and an exact reference in Cochran and Cox, are listed for each plan. If the data from a farm experiment are sent to a consulting biometrician for analysis, the pertinent information from this table, together with a copy of the plan, should be included.

One word more about the statistical analysis: Remember that in most of the plans a complete set of treatments is not tested on every farm. Therefore, before you can make comparisons, you must equalize treatment values to take into account the differences among farms. Do not make the mistake of taking the degrees of freedom shown in Table 6 and proceeding as if the tests were simply designed as randomized blocks.

11/ Cochran and Cox, *op. cit.*

Table 6. Notes for statistical analysis: Degrees of freedom for the analysis of variance and the pertinent reference in Cochran and Cox, Experimental Designs.

Abbreviations: T = No. of treatments
R = No. of repetitions of plan
L = No. of locations
A = No. of arrangements used
X = No. of times the selected number of arrangements are repeated

Plan	Degrees of freedom**				Reference in Cochran and Cox
	Treatments	Locations	Error	Total	
A	T-1	-	T(R - 1)	L-1	Sect. 4.1
B	T-1	L-1	(T-1)(L-1)	TL-1	Sect. 4.2
C	T	-	T(R-1)	L	Sect. 4.1*
D1	2	3R-1	3R-2	6R-1	Sect. 11.54
D2	3	6R-1	6R-3	12R-1	Plan 11.1
D3	4	10R-1	10R-4	20R-1	Plan 11.2
D4	5	15R-1	15R-5	30R-1	Plan 11.3
D5	6	21R-1	21R-6	42R-1	Table 11.3
D6	7	28R-1	28R-7	56R-1	Plan 11.9
D7	8	36R-1	36R-8	72R-1	Table 11.3
D8	9	45R-1	45R-9	90R-1	Plan 11.14

See footnotes at end of table.

Table 6. Notes for statistical analysis: Degrees of freedom for the analysis of variance and the pertinent reference in Cochran and Cox, Experimental Designs--Continued.

Plan	Degrees of freedom**				Reference in Cochran and Cox
	Treatments	Locations	Error	Total	
E1	3	4R-1	8R-3	12R-1	Table 11.3
E2	4	10R-1	20R-4	30R-1	Table 11.3
E3	5	10R-1	20R-5	30R-1	Plan 11.4
E4	6	7R-1	14R-6	21R-1	Plan 11.7
E5	8	12R-1	24R-8	36R-1	Plan 10.1
E6	9	30R-1	60R-9	90R-1	Plan 11.15
F1	4	5R-1	15R-4	20R-1	Table 11.3
F2	5	15R-1	45R-5	60R-1	Plan 11.6
F3	6	7R-1	21R-6	28R-1	Plan 11.8
F4	7	14R-1	42R-7	56R-1	Plan 11.10
F5	8	18R-1	54R-8	72R-1	Plan 11.11
F6	9	15R-1	45R-9	60R-1	Plan 11.16
G1	15	4AX-1	12AX-15	16AX-1	Plan 10.2
G2	24	5AX-1	20AX-24	25AX-1	Plan 10.3
G3	26	27R-1	54R-26	81R-1	Sect. 10.4
G4	35	6AX-1	30AX-35	36AX-1	Plan 10.7

See footnotes at end of table.

Table 6. Notes for statistical analysis: Degrees of freedom for the analysis of variance and the pertinent reference in Cochran and Cox, Experimental Designs--Continued.

Plan	Degrees of freedom**				Reference in Cochran and Cox
	Treatments	Locations	Error	Total	
H1	3	6R-1	6R-3	12R-1	Plan 11.1
H2	3	R-1	3R-3	4R-1	Sect. 5.1
H3	8	6R-1	12R-8	18R-1	Sect. 6.15
H4	8	R-1	8R-8	9R-1	Sect. 5.26
H5	15	12R-1	36R-15	48R-1	Plan 6.12
H6	6 ***	2R-1	6R-6	8R-1	Plan 6.1
H7	7	R-1	7R-7	8R-1	Sect. 5.23
H8	26	3AX-1	24AX-26	27AX-1	Plan 6.7
H9	15	4AX-1	12AX-15	16AX-1	Plan 6.4
H10	14***	2R-1	14R-14	16R-1	Plan 6.2

* Analysis is made on differences at each location between check and treated plots.

** For designs that involve subdivision of a region (see Step 2, pages 21-26), locations should be partitioned into subdivisions and farms in subdivisions, while the error should be partitioned into treatments x subdivisions and treatments x farms in subdivisions.

*** One degree of freedom confounded with locations.

APPENDIX: THE PLANS

Plan A

2 or more separate treatments.
1 treatment and 1 plot on each farm.
A single repetition of the plan requires as many farms as there are treatments and contains 1 replication.

Farm 1	Farm 2	
Treatment 1	Treatment 2	. . . and so forth

Plan B

2 or more separate treatments.
A block of all treatments on each farm.
A single repetition of the plan appears on each farm and contains 1 replication.

Block 1	Block 2	Block 3	Block 4
1	1	1	1
2	2	2	2
etc.	etc.	etc.	etc.

. . . and so forth.

Plan C

2 or more separate treatments, each compared with a check.

A block of 2 plots on each farm--one for a treatment and the other for the check.

A single repetition of the plan requires as many farms as there are treatments and contains 1 replication.

Block 1	Block 2	
1	2	
check	check	. . . and so forth.

Plan D-1

3 separate treatments.
A block of 2 plots (treatments) on each farm.
A single repetition of the plan requires 3 farms and contains 2 replications.

Block 1	Block 2	Block 3
1	1	2
2	3	3

Plan D-2

4 separate treatments.
A block of 2 plots (treatment) on each farm.
A single repetition of the plan requires 6 farms and contains 3 replications.

Block 1	Block 2	Block 3
1	1	1
2	3	4

Block 4	Block 5	Block 6
2	2	3
3	4	4

Plan D-3

5 separate treatments.
A block of 2 plots (treatments) on each farm.
A single repetition of the plan requires 10 farms and contains 4 replications.

Block 1	Block 2	Block 3	Block 4	Block 5
1	1	1	1	2
2	3	4	5	3

Block 6	Block 7	Block 8	Block 9	Block 10
2	2	3	3	4
4	5	4	5	5

Plan D-4

6 separate treatments.
A block of 2 plots (treatments) on each farm.
A single repetition of the plan requires 15 farms and contains
5 replications.

Block 1	Block 2	Block 3	Block 4	Block 5
1	1	1	1	1
2	3	4	5	6

Block 6	7	8	9	10
2	2	2	2	3
3	4	5	6	4

Block 11	12	13	14	15
3	3	4	4	5
5	6	5	6	6

Plan D-5

7 separate treatments
A block of 2 plots (treatments) on each farm.
A single repetition of the plan requires 21 farms and contains
6 replications.

Block 1	Block 2	Block 3	Block 4	Block 5	Block 6	Block 7
1	1	1	1	1	1	2
2	3	4	5	6	7	3

Block 8	9	10	11	12	13	14
2	2	2	2	3	3	3
4	5	6	7	4	5	6

Block 15	16	17	18	19	20	21
3	4	4	4	5	5	6
7	5	6	7	6	7	7

Plan D-6

8 treatments.
A block of 2 plots (treatments) on each farm.
A single repetition of the plan requires 28 farms and contains
7 replications.

Block 1	Block 2	Block 3	Block 4	Block 5	Block 6	Block 7
1	1	1	1	1	1	1
2	3	4	5	6	7	8

Block 8	9	10	11	12	13	14
2	2	2	2	2	2	3
3	4	5	6	7	8	4

Block 15	16	17	18	19	20	21
3	3	3	3	4	4	4
5	6	7	8	5	6	7

Block 22	23	24	25	26	27	28
4	5	5	5	6	6	7
8	6	7	8	7	8	8

Plan D-7

9 treatments.
A block of 2 plots (treatments) per farm.
A single repetition of the plan requires 36 locations and contains 8 replications.

Block 1	Block 2	Block 3	Block 4	Block 5	Block 6	Block 7	Block 8	Block 9
1	1	1	1	1	1	1	1	2
2	3	4	5	6	7	8	9	3

Block 10	11	12	13	14	15	16	17	18
2	2	2	2	2	2	3	3	3
4	5	6	7	8	9	4	5	6

Block 19	20	21	22	23	24	25	26	27
3	3	3	4	4	4	4	4	5
7	8	9	5	6	7	8	9	6

Block 28	29	30	31	32	33	34	35	36
5	5	5	6	6	6	7	7	8
7	8	9	7	8	9	8	9	9

Plan D-8

10 separate treatments.
A block of 2 plots (treatments) on each farm.
A single repetition of the plan requires 45 farms and contains 9 replications.

Block 1	Block 2	Block 3	Block 4	Block 5	Block 6	Block 7	Block 8	Block 9
1	1	1	1	1	1	1	1	1
2	3	4	5	6	7	8	9	10

Block 10	11	12	13	14	15	16	17	18
2	2	2	2	2	2	2	2	3
3	4	5	6	7	8	9	10	4

Block 19	20	21	22	23	24	25	26	27
3	3	3	3	3	3	4	4	4
5	6	7	8	9	10	5	6	7

Block 28	29	30	31	32	33	34	35	36
4	4	4	5	5	5	5	5	6
8	9	10	6	7	8	9	10	7

Block 37	38	39	40	41	42	43	44	45
6	6	6	7	7	7	8	8	9
8	9	10	8	9	10	9	10	10

Plan E-1

4 separate treatments.
A block of 3 plots (treatments) on each farm.
A single repetition of the plan requires 4 farms and contains
3 replications.

Block 1	Block 2	Block 3	Block 4
1	1	1	2
2	2	3	3
3	4	4	4

Plan E-2

5 separate treatments.
A block of 3 plots (treatments) on each farm.
A single repetition of the plan requires 10 farms and contains
6 replications.

Block 1	Block 2	Block 3	Block 4	Block 5
1	1	1	1	1
2	2	2	3	3
3	4	5	4	5

Block 6	Block 7	Block 8	Block 9	Block 10
1	2	2	2	3
4	3	3	4	4
5	4	5	5	5

Plan E-3

6 separate treatments.
A block of 3 plots (treatments) on each farm.
A single repetition of the plan requires 10 farms and contains 5 replications

Block 1	Block 2	Block 3	Block 4	Block 5
1	1	1	1	1
2	2	3	3	4
5	6	4	6	5

Block 6	Block 7	Block 8	Block 9	Block 10
2	2	2	3	4
3	3	4	5	5
4	5	6	6	6

Plan E-4

7 separate treatments.
A block of 3 plots (treatments) on each farm.
A single repetition of the plan requires 7 farms and contains 3 replications.

Block 1	Block 2	Block 3	Block 4	Block 5	Block 6	Block 7
1	2	3	4	5	6	7
2	3	4	5	6	7	1
4	5	6	7	1	2	3

Plan E-5

9 separate treatments.
A block of 3 plots (treatments) on each farm.
A single repetition of the plan requires 12 farms and contains 4 replications.

Block 1	Block 2	Block 3	Block 4	Block 5	Block 6
1	4	7	1	2	3
2	5	8	4	5	6
3	6	9	7	8	9

Block 7	Block 8	Block 9	Block 10	Block 11	Block 12
1	7	4	1	4	7
5	2	8	8	2	5
9	6	3	6	9	3

Plan E-6

10 separate treatments.
A block of 3 plots (treatments) on each farm.
A single repetition of the plan requires 30 farms and contains
9 replications.

Block 1	Block 2	Block 3	Block 4	Block 5	Block 6	Block 7	Block 8	Block 9	Block 10
1	1	1	1	1	1	1	1	1	2
2	2	3	4	5	6	7	8	9	3
3	4	5	6	7	8	9	10	10	6

Block 11	12	13	14	15	16	17	18	19	20
2	2	2	2	2	2	3	3	3	3
4	5	5	6	7	8	4	4	5	7
10	8	9	7	9	10	7	8	6	10

Block 21	22	23	24	25	26	27	28	29	30
3	3	4	4	4	4	5	5	6	6
8	9	5	5	6	7	6	7	7	8
9	10	9	10	9	8	10	8	10	9

Plan F-1

5 separate treatments.
A block of 4 plots (treatments) on each farm.
A single repetition of the plan requires 5 farms and contains 4 replications.

Block 1	Block 2	Block 3	Block 4	Block 5
1	1	1	1	2
2	2	2	3	3
3	3	4	4	4
4	5	5	5	5

Plan F-2

6 separate treatments.
A block of 4 plots (treatments) on each farm.
A single repetition of the plan requires 15 farms and contains 10 replications

Block 1	Block 2	Block 3	Block 4	Block 5
1	1	1	1	1
2	2	2	2	2
3	3	3	4	4
4	5	6	5	6

Block 6	7	8	9	10
1	1	1	1	1
2	3	3	3	4
5	4	4	5	5
6	5	6	6	6

Block 11	12	13	14	15
2	2	2	2	3
3	3	3	4	4
4	4	5	5	5
5	6	6	6	6

Plan F-3

7 separate treatments.
A block of 4 plots (treatments) on each farm.
A single repetition of the plan requires 7 farms and contains 4 replications.

Block 1	Block 2	Block 3	Block 4	Block 5	Block 6	Block 7
1	1	1	1	2	2	3
2	2	3	4	3	4	5
3	5	4	6	4	5	6
6	7	5	7	7	6	7

Plan F-4

8 separate treatments.
A block of 4 plots (treatments) on each farm.
A single repetition of the plan requires 14 farms and contains 7 replications.

Block 1	Block 2	Block 3	Block 4	Block 5	Block 6	Block 7
1	5	1	3	1	3	1
2	6	2	4	2	4	3
3	7	5	7	7	5	5
4	8	6	8	8	6	7

Block 8	9	10	11	12	13	14
2	1	2	1	2	1	2
4	3	4	4	3	4	3
6	6	5	5	6	6	5
8	8	7	8	7	7	8

Plan F-5

9 treatments.
A block of 4 plots (treatments) on each farm.
A single repetition of the plan requires 18 farms and contains
8 replications.

Block 1	Block 2	Block 3	Block 4	Block 5	Block 6
1	1	1	1	1	1
2	2	2	3	4	3
3	5	7	5	6	6
4	6	8	7	8	9

Block 7	8	9	10	11	12
1	1	2	2	2	2
4	5	3	4	6	3
8	7	8	5	7	4
9	9	9	9	9	7

Block 13	14	15	16	17	18
2	3	4	3	3	4
5	5	6	4	6	5
6	8	7	5	7	7
8	9	9	6	8	8

Plan F-6

10 separate treatments.
A block of 4 plots (treatments) on each farm.
A single repetition of the plan requires 15 farms and contains 6 replications.

Block 1	Block 2	Block 3	Block 4	Block 5
1	1	1	1	1
2	2	3	4	5
3	5	7	9	7
4	6	8	10	9

Block 6	7	8	9	10
1	2	2	2	2
6	3	4	5	7
8	6	7	8	8
10	9	10	10	9

Block 11	12	13	14	15
3	3	3	4	4
5	6	4	5	6
9	7	5	6	8
10	10	8	7	9

Plan G-1

16 treatments.
A block of 4 plots (treatments) on each farm.
A single repetition of the plan requires 4 farms and contains 1 replication.

Note that for each repetition 5 arrangements are possible. First determine the number of repetitions you need (use Step 3, pages 15-17), and then get them in by using as <u>many</u> arrangements as you can, repeating them as many times as you need to. For example--

1. For 15 repetitions, use 5 arrangements 3 times--not 3 arrangements 5 times.
*2. For 7 repetitions, use 4 arrangements 2 times--not 2 arrangements 4 times.
3. For 2 repetitions, use 2 arrangements 1 time--not 1 arrangement 2 times.

<u>Arrangement 1</u>

Block

1	2	3	4
1	5	9	13
2	6	10	14
3	7	11	15
4	8	12	16

<u>Arrangement 2</u>

Block

1	2	3	4
1	2	3	4
5	6	7	8
9	10	11	12
13	14	15	16

<u>Arrangement 3</u>

Block

1	2	3	4
1	5	9	13
6	2	14	10
11	15	3	7
16	12	8	4

<u>Arrangement 4</u>

Block

1	2	3	4
1	13	5	9
14	2	10	6
7	11	3	15
12	8	16	4

<u>Arrangement 5</u>

Block

1	2	3	4
1	9	13	5
10	2	6	14
15	7	3	11
8	16	12	4

* Note: Number of repetitions is raised to 8 because 7 is prime.

Plan G-2

25 separate treatments.
A block of 5 plots (treatments) on each farm.
A single repetition of the plan requires 5 farms and contains
1 replication.

Note that for each repetition 6 arrangements are possible. First determine the number of repetitions you need (use Step 3, pages 15-17), and then get them in by using as <u>many</u> arrangements as you can, repeating them as many times as you need to. For Example--

1. For 18 repetitions, use 6 arrangements 3 times--not 3 arrangements 6 times, or 2 arrangements 9 times.
*2. For 7 repetitions, use 4 arrangements 2 times--not 2 arrangements 4 times.
3. For 2 repetitions, use 2 arrangements 1 time--not 1 arrangement 2 times.

Arrangement 1

Block

1	2	3	4	5
1	6	11	16	21
2	7	12	17	22
3	8	13	18	23
4	9	14	19	24
5	10	15	20	25

Arrangement 2

Block

1	2	3	4	5
1	2	3	4	5
6	7	8	9	10
11	12	13	14	15
16	17	18	19	20
21	22	23	24	25

Arrangement 3

Block

1	2	3	4	5
1	21	16	11	6
7	2	22	17	12
13	8	3	23	18
19	14	9	4	24
25	20	15	10	5

Arrangement 4

Block

1	2	3	4	5
1	16	6	21	11
12	2	17	7	22
23	13	3	18	8
9	24	14	4	19
20	10	25	15	5

Arrangement 5

Block

1	2	3	4	5
1	11	21	6	16
17	2	12	22	7
8	18	3	13	23
24	9	19	4	14
15	25	10	20	5

Arrangement 6

Block

1	2	3	4	5
1	6	11	16	21
22	2	7	12	17
18	23	3	8	13
14	19	24	4	9
10	15	20	25	5

* Note: Number of repetitions is raised to 8 because 7 is prime.

Plan G-3

27 separate treatments.
A block of 3 plots (treatments) on each farm.
A single repetition of the plan requires 27 farms and contains 3 replications.

Block 1	Block 2	Block 3	Block 4	Block 5	Block 6	Block 7	Block 8	Block 9
1	4	7	10	13	16	19	22	25
2	5	8	11	14	17	20	23	26
3	6	9	12	15	18	21	24	27

Block 10	11	12	13	14	15	16	17	18
1	2	3	10	11	12	19	20	21
4	5	6	13	14	15	22	23	24
7	8	9	16	17	18	25	26	27

Block 19	20	21	22	23	24	25	26	27
1	2	3	4	5	6	7	8	9
10	11	12	13	14	15	16	17	18
19	20	21	22	23	24	25	26	27

36 separate treatments.
A block of 6 plots (treatments) on each farm.
A single repetition of the plan requires 6 farms and contains only 1 replication for each treatment.

Note that for each repetition 3 arrangements are possible, and that each arrangement constitutes 1 repetition. First determine the number of repetitions you need (use Step 3, pages 15-17), and then get them in by using as many arrangements as you can, repeating them as many times as you need to. For example--

1. For 12 repetitions, use 3 arrangements 4 times--not 2 arrangements 6 times.
*2. For 5 repetitions, use 3 arrangements 2 times--not 2 arrangements 3 times.
3. For 2 repetitions, use 2 arrangements 1 time--not 1 arrangement 2 times.

Arrangement 1

Block

1	2	3	4	5	6
1	7	13	19	25	31
2	8	14	20	26	32
3	9	15	21	27	33
4	10	16	22	28	34
5	11	17	23	29	35
6	12	18	24	30	36

Arrangement 2

Block

1	2	3	4	5	6
1	2	3	4	5	6
7	8	9	10	11	12
13	14	15	16	17	18
19	20	21	22	23	24
25	26	27	28	29	30
31	32	33	34	35	36

Arrangement 3

Block

1	2	3	4	5	6
1	31	25	19	13	7
8	2	32	26	20	14
15	9	3	33	27	21
22	16	10	4	34	28
29	23	17	11	5	35
36	30	24	18	12	6

* Note: Number of repetitions is raised to 6 because 5 is prime.

Plan H-1

4 treatments: All possible combinations of 2 factors (a, b) at 2 levels (lower (1) and upper (2)). The lower level of a factor may be the complete absence of it.
A block of 2 plots (treatments) on each farm.
A single repetition of the plan requires 6 farms and contains 3 replications.

Block 1	Block 2	Block 3	Block 4	Block 5	Block 6
a_1b_1	a_1b_1	a_1b_1	a_2b_1	a_2b_1	a_1b_2
a_2b_1	a_1b_2	a_2b_2	a_1b_2	a_2b_2	a_2b_2

Plan H-2

4 treatments: All possible combinations of 2 factors (a, b) at 2 levels (lower (1) and upper (2)). The lower level of a factor may be the complete absence of it.
A block of treatments (4 plots) at each farm.
A single repetition of the plan requires 1 farm and, for main effects, contains 2 replications.

Block 1

a_1b_1
a_2b_1
a_1b_2
a_2b_2

Plan H-3

9 treatments: All possible combinations of 2 factors (a, b) at 3 levels (lowest (1), middle (2), and highest (3)). The lowest level of a factor may be the complete absence of it and may correspond to a check treatment.
A block of 3 plots (treatments) on each farm.
Use at least 2 repetitions of this plan. A single repetition requires 6 farms and, for main effects, contains 6 replications.

Block 1	Block 2	Block 3
a_1b_1	a_1b_2	a_1b_3
a_2b_2	a_2b_3	a_2b_1
a_3b_3	a_3b_1	a_3b_2

Block 4	Block 5	Block 6
a_1b_1	a_1b_2	a_1b_3
a_2b_3	a_2b_1	a_2b_2
a_3b_2	a_3b_3	a_3b_1

Plan H-4

9 treatments: All possible combinations of 2 factors (a, b) at 3 levels (lowest (1), middle (2), and highest (3)). The lowest level of a factor may be the complete absence of it and may correspond to a check treatment.

A block of treatments (9 plots) on each farm.

Use at least 2 repetitions of this plan. A single repetition requires 1 farm and, for main effects, contains 3 replications.

Block 1

a_1b_1
a_2b_1
a_3b_1
a_1b_2
a_2b_2
a_3b_2
a_1b_3
a_2b_3
a_3b_3

Plan H-5

16 treatments: All possible combinations of 2 factors (a, b) at 4 levels. The lowest level of a factor may be the complete absence of it and may correspond to a check treatment.
A block of 4 plots (treatments) on each farm.
A single repetition of the plan requires 12 farms and, for main effects, contains 12 replications.

Block 1	Block 2	Block 3	Block 4
a_4b_4	a_4b_3	a_4b_2	a_4b_1
a_3b_3	a_3b_4	a_3b_1	a_3b_2
a_2b_1	a_2b_2	a_2b_3	a_2b_4
a_1b_2	a_1b_1	a_1b_4	a_1b_3

Block 5	6	7	8
a_4b_4	a_4b_1	a_4b_3	a_4b_2
a_3b_2	a_3b_3	a_3b_1	a_3b_4
a_2b_3	a_2b_2	a_2b_4	a_2b_1
a_1b_1	a_1b_4	a_1b_2	a_1b_3

Block 9	10	11	12
a_4b_4	a_4b_2	a_4b_3	a_4b_1
a_3b_1	a_3b_3	a_3b_2	a_3b_4
a_2b_2	a_2b_4	a_2b_1	a_2b_3
a_1b_3	a_1b_1	a_1b_4	a_1b_2

Plan H-6

8 treatments: All possible combinations of 3 factors (a, b, c) at 2 levels (lower (1) and upper (2)). The lower level of a factor may be the complete absence of it and may correspond to a check treatment.

A block of 4 plots (treatments) on each farm.

Use at least 2 repetitions of this plan. A single repetition requires 2 farms and, for main effects, contains 4 replications.

Block 1	Block 2
$a_2b_1c_1$	$a_2b_2c_1$
$a_1b_2c_1$	$a_2b_1c_2$
$a_1b_1c_2$	$a_1b_2c_2$
$a_2b_2c_2$	$a_1b_1c_1$

Plan H-7

8 treatments: All possible combinations of 3 factors (a, b, c) at 2 levels (upper (1) and lower (2)). The lower level of a factor may be the complete absence of it and may correspond to a check treatment.

A block of all treatments (8 plots) on each farm.

Use at least 2 repetitions of this plan. A single repetition requires 1 farm and, for main effects, contains 4 replications.

Block 1

$a_1b_1c_1$
$a_2b_1c_1$
$a_1b_2c_1$
$a_2b_2c_1$
$a_1b_1c_2$
$a_2b_1c_2$
$a_1b_2c_2$
$a_2b_2c_2$

Plan H-8

27 treatments: All possible combinations of 3 factors (a, b, c) at 3 levels (lowest (1), middle (2), and highest (3)). The lowest level of a factor may be its complete absence and may correspond to a check treatment.

A block of 9 (treatments) on each farm.

A single repetition of the plan requires 3 farms and, for main effects, contains 9 replications.

Note that for each repetition 4 arrangements are possible and that each arrangement constitutes 1 repetition. First determine the number of repetitions you need (use Step 3, pages 15-17), and then get them in by using as many arrangements as you can (use at least the first 2), repeating them as many times as you need to. For example--

1. For 7 repetitions, use 4 arrangements 2 times*--not 2 arrangements 4 times, or 1 arrangement 7 times.
2. For 3 repetitions, use 3 arrangements 1 time--not 1 arrangement 3 times.

Arrangement 1			Arrangement 2		
Block 1	Block 2	Block 3	Block 1	Block 2	Block 3
$a_1b_1c_1$	$a_1b_1c_2$	$a_1b_1c_3$	$a_1b_1c_1$	$a_1b_1c_2$	$a_1b_1c_3$
$a_1b_2c_2$	$a_1b_2c_3$	$a_1b_2c_1$	$a_1b_2c_3$	$a_1b_2c_1$	$a_1b_2c_2$
$a_1b_3c_3$	$a_1b_3c_1$	$a_1b_3c_2$	$a_1b_3c_2$	$a_1b_3c_3$	$a_1b_3c_1$
$a_2b_1c_2$	$a_2b_1c_3$	$a_2b_1c_1$	$a_2b_1c_3$	$a_2b_1c_1$	$a_2b_1c_2$
$a_2b_2c_3$	$a_2b_2c_1$	$a_2b_2c_2$	$a_2b_2c_2$	$a_2b_2c_3$	$a_2b_2c_1$
$a_2b_3c_1$	$a_2b_3c_2$	$a_2b_3c_3$	$a_2b_3c_1$	$a_2b_3c_2$	$a_2b_3c_3$
$a_3b_1c_3$	$a_3b_1c_1$	$a_3b_1c_2$	$a_3b_1c_2$	$a_3b_1c_3$	$a_3b_1c_1$
$a_3b_2c_1$	$a_3b_2c_2$	$a_3b_2c_3$	$a_3b_2c_1$	$a_3b_2c_2$	$a_3b_2c_3$
$a_3b_3c_2$	$a_3b_3c_3$	$a_3b_3c_1$	$a_3b_3c_3$	$a_3b_3c_1$	$a_3b_3c_2$

* Note: Number of repetitions is raised to 8 because 7 is prime.

Plan H-8 -- Continued

Arrangement 3			Arrangement 4		
Block 1	Block 2	Block 3	Block 1	Block 2	Block 3
$a_1b_1c_1$	$a_1b_1c_2$	$a_1b_1c_3$	$a_1b_1c_1$	$a_1b_1c_2$	$a_1b_1c_3$
$a_1b_2c_2$	$a_1b_2c_3$	$a_1b_2c_1$	$a_1b_2c_3$	$a_1b_2c_1$	$a_1b_2c_2$
$a_1b_3c_3$	$a_1b_3c_1$	$a_1b_3c_2$	$a_1b_3c_2$	$a_1b_3c_3$	$a_1b_3c_1$
$a_2b_1c_3$	$a_2b_1c_1$	$a_2b_1c_2$	$a_2b_1c_2$	$a_2b_1c_3$	$a_2b_1c_1$
$a_2b_2c_1$	$a_2b_2c_2$	$a_2b_2c_3$	$a_2b_2c_1$	$a_2b_2c_2$	$a_2b_2c_3$
$a_2b_3c_2$	$a_2b_3c_3$	$a_2b_3c_1$	$a_2b_3c_3$	$a_2b_3c_1$	$a_2b_3c_2$
$a_3b_1c_2$	$a_3b_1c_3$	$a_3b_1c_1$	$a_3b_1c_3$	$a_3b_1c_1$	$a_3b_1c_2$
$a_3b_2c_3$	$a_3b_2c_1$	$a_3b_2c_2$	$a_3b_2c_2$	$a_3b_2c_3$	$a_3b_2c_1$
$a_3b_3c_1$	$a_3b_3c_2$	$a_3b_3c_3$	$a_3b_3c_1$	$a_3b_3c_2$	$a_3b_3c_3$

16 treatments: All possible combinations of 4 factors (a, b, c, d) at 2 levels (lower (1) and upper (2)). The lower level of a factor may be its complete absence and may correspond to a check treatment.

A block of 4 plots (treatments) on each farm.

A single repetition of the plan requires 4 farms and, for main effects, contains 8 replications.

Note that for each repetition 6 arrangements are possible and that each arrangement constitutes 1 repetition. First determine the number of repetitions you need (use Step 3, pages 15-17), and then get them in by using as <u>many</u> arrangements as you can (use at least the first 2), repeating them as many times as you need to. For example--

1. For 11 repetitions, use 6 arrangements 2 times*--not 4 arrangements 3 times, 3 arrangements 4 times, 2 arrangements 6 times, or 1 arrangement 11 times.
2. For 4 repetitions, use 4 arrangements 1 time--not 1 arrangement 4 times.

<u>Arrangement 1</u>

Block 1	Block 2	Block 3	Block 4
$a_1b_1c_1d_1$	$a_1b_1c_2d_2$	$a_1b_1c_2d_1$	$a_1b_1c_1d_2$
$a_2b_1c_2d_2$	$a_2b_1c_1d_1$	$a_2b_1c_1d_2$	$a_2b_1c_2d_1$
$a_1b_2c_2d_2$	$a_1b_2c_1d_1$	$a_1b_2c_1d_2$	$a_1b_2c_2d_1$
$a_2b_2c_1d_1$	$a_2b_2c_2d_2$	$a_2b_2c_2d_1$	$a_2b_2c_1d_2$

<u>Arrangement 2</u>

$a_1b_1c_1d_1$	$a_2b_2c_1d_1$	$a_2b_1c_1d_1$	$a_1b_2c_1d_1$
$a_2b_2c_2d_1$	$a_1b_1c_2d_1$	$a_1b_2c_2d_1$	$a_2b_1c_2d_1$
$a_2b_2c_1d_2$	$a_1b_1c_1d_2$	$a_1b_2c_1d_2$	$a_2b_1c_1d_2$
$a_1b_1c_2d_2$	$a_2b_2c_2d_2$	$a_2b_1c_2d_2$	$a_1b_2c_2d_2$

* Note: Number of repetitions is raised to 12 because 11 is prime.

Arrangement 3

Block 1	Block 2	Block 3	Block 4
$a_1b_1c_1d_1$	$a_1b_2c_1d_2$	$a_1b_2c_1d_1$	$a_1b_1c_1d_2$
$a_2b_2c_1d_2$	$a_2b_1c_1d_1$	$a_2b_1c_1d_2$	$a_2b_2c_1d_1$
$a_1b_2c_2d_2$	$a_1b_1c_2d_1$	$a_1b_1c_2d_2$	$a_1b_2c_2d_1$
$a_2b_1c_2d_1$	$a_2b_2c_2d_2$	$a_2b_2c_2d_1$	$a_2b_1c_2d_2$

Arrangement 4

Block 1	Block 2	Block 3	Block 4
$a_1b_1c_1d_1$	$a_1b_2c_2d_1$	$a_1b_2c_1d_1$	$a_1b_1c_2d_1$
$a_2b_2c_2d_1$	$a_2b_1c_1d_1$	$a_2b_1c_2d_1$	$a_2b_2c_1d_1$
$a_1b_2c_2d_2$	$a_1b_1c_1d_2$	$a_1b_1c_2d_2$	$a_1b_2c_1d_2$
$a_2b_1c_1d_2$	$a_2b_2c_2d_2$	$a_2b_2c_1d_2$	$a_2b_1c_2d_2$

Arrangement 5

Block 1	Block 2	Block 3	Block 4
$a_1b_1c_1d_1$	$a_2b_1c_1d_2$	$a_2b_1c_1d_1$	$a_1b_1c_1d_2$
$a_2b_2c_1d_2$	$a_1b_2c_1d_1$	$a_1b_2c_1d_2$	$a_2b_2c_1d_1$
$a_2b_1c_2d_2$	$a_1b_1c_2d_1$	$a_1b_1c_2d_2$	$a_2b_1c_2d_1$
$a_1b_2c_2d_1$	$a_2b_2c_2d_2$	$a_2b_2c_2d_1$	$a_1b_2c_2d_2$

Arrangement 6

Block 1	Block 2	Block 3	Block 4
$a_1b_1c_1d_1$	$a_2b_1c_2d_1$	$a_2b_1c_1d_1$	$a_1b_1c_2d_1$
$a_2b_2c_2d_1$	$a_1b_2c_1d_1$	$a_1b_2c_2d_1$	$a_2b_2c_1d_1$
$a_2b_1c_2d_2$	$a_1b_1c_1d_2$	$a_1b_1c_2d_2$	$a_2b_1c_1d_2$
$a_1b_2c_1d_2$	$a_2b_2c_2d_2$	$a_2b_2c_1d_2$	$a_1b_2c_2d_2$

Plan H-10

16 treatments: All possible combinations of 4 factors (a, b, c, d) at 2 levels (lower (1) and upper (2)). The lower level of a factor may be its complete absence and may correspond to a check treatment.
A block of 8 plots (treatments) on each farm.
A single repetition of the plan requires 2 farms and, for main effects, contains 8 replications. Use at least 2 repetitions.

Block 1	Block 2
$a_2b_1c_1d_1$	$a_1b_1c_1d_1$
$a_1b_2c_1d_1$	$a_2b_2c_1d_1$
$a_1b_1c_2d_1$	$a_2b_1c_2d_1$
$a_1b_1c_1d_2$	$a_1b_2c_2d_1$
$a_2b_2c_2d_1$	$a_2b_1c_1d_2$
$a_2b_2c_1d_2$	$a_1b_2c_1d_2$
$a_2b_1c_2d_2$	$a_1b_1c_2d_2$
$a_1b_2c_2d_2$	$a_2b_2c_2d_2$

A Guide to

extensive testing on farms

in four parts

PART IV.

Foreign Agricultural Service April 1954
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a guide to

EXTENSIVE TESTING ON FARMS

by Henry Hopp

in 4 parts

Part IV: Using Data to Design Extensive Tests on Farms

Foreign Agricultural Service
UNITED STATES DEPARTMENT OF AGRICULTURE
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CONTENTS

PREFACE

Parts II and III of this Guide describe procedures for laying out the two kinds of extensive tests--result tests and experiments on farms. One of the important items in the cost of such projects is their size, i.e., the number of farms they involve and the area of the plots. Of course you want to keep the undertaking as small as possible, and yet you want to meet the requirements for adequate design. In the previous parts, some rather crude methods were given for deciding on the number of farms to include in an extensive test: Part II, on result tests, gave a rule-of-thumb method; Part III, on farm experiments, gave a somewhat better method--making "informed guesses" and applying to them certain simple statistical procedures.

Neither of these methods is likely to be very precise. They might miss the mark by a great deal. The one source of really accurate information would be data from surveys or experiments.

Fortunately such data are often available: surveys already may have been made in the area; experiments already may have been conducted at one or several research stations. If such data pertinent to an extensive test are available, they can be used in estimating the number of farms as well as the size of plots. The methods involve conventional statistical procedures and therefore require some knowledge of statistical analysis. For tests that are critical, costly, or time-consuming, the added inconvenience that statistical methods entail may be minor compared with the savings they accomplish.

This part of the Guide (1) describes the procedure for using data in estimating the number of farms required for the extensive test, (2) tells how to modify this procedure when you want plots larger than the research plots, and (3) gives a method for determining the <u>best</u> size for extensive-test plots.

This part, then, serves as a supplement to the three preceding parts. You will need it only when you wish to improve the design that you have already arrived at through the previous parts; and the directions that you find here can be used only in connection with that design.

IMPROVING THE ESTIMATE OF NUMBER OF FARMS REQUIRED

In Part III of this Guide you learned how to decide on the number of farms to have in an extensive test. You estimated three kinds of variability--plot, location, and treatment--and you learned to do it by a method we might dignify as "informed guessing." With this rather crude method, your decision as to the number of farms will not be completely reliable, but you will have to be satisfied with it if you lack actual yield data from surveys or experiments. If, however, you have or can get some pertinent yield data and think it worthwhile to determine the number of farms more reliably, you can substitute more accurate methods for estimating each of the three kinds of variability.

The method to use will depend on, first, the kind of variability you are estimating; second, the source of your data, i.e., whether they are from surveys or from experiments; and third, the competence and facilities available for making statistical computations, i.e., whether you have a trained technician and a calculating machine for him to work with. Three methods are presented here.

Use Method 1 for plot or location variability if you have, or can obtain, survey data on yields and if you do not have a technician and machine available.

Use Method 2 for plot or location variability if you have, or can obtain, survey data and if you have a technician and machine.

Use Method 3 for plot, location, or treatment variability if you have data from experiments in the area and if you have a technician and machine.

Although each of these methods will give you a more precise estimate of the optimum number of farms than the guessing method, the third one will give you the most precise estimate of all. This method, however, can be used only if some experiments already have been performed in your area on the practice you are testing. For Methods 1 and 2, usually, data are more easily obtained.

You do not have to use the same method for estimating all three kinds of variability. Thus, you might be planning an extensive test on corn fertilizers, and, though you may not have data on the subject, you may have some that were obtained in experiments with corn varieties. You can use these data for determining plot variability, following Method 3. But, if the research had been done at only two or three locations, the data may be insufficient for getting location variability. So, for location variability, you make a little survey and obtain an estimate by Method 2. Finally, lacking specific information on corn fertilizer experiments, you may have to fall back on the guess procedure in Part III of this Guide to estimate treatment variability.

PLOT VARIABILITY

Method 1

Method 1 uses survey data and requires no calculating machine. Perhaps you already have data from surveys previously made; but, if you do not, you can get some by collecting simple crop-cutting samples from 2 adjoining plots on each of 10 or more farms, having each plot of the same size as plots you later will use in the extensive test. From such data you can make a fair approximation of plot variability, even without a calculating machine.

Let us consider an example. You have collected data, let us say, on yields from 2 adjoining plots on each of 15 farms. Now, for each farm enter the yield for each plot in columns 2 and 3 of table 1; in column 4 enter the difference between them. When you have done this for all farms, use column 5 to number the differences consecutively in increasing order of magnitude. Thus, Farm 5, which has the smallest difference between plots, get the rank of 1, and Farm 2, which has the largest difference, gets the rank of 15. Now list the 15 farms in the order of this rank (table 2). The midpoint, or median, rank is 8; and the difference corresponding to this rank is 6. Now use this difference to obtain the figure for plot variability, thus:

$$\text{Plot variability} = \frac{\text{Difference at midpoint rank}}{1.4 \ \text{1/}} = \frac{6}{1.4} = 4.3$$

To express this plot variability in percent you must first calculate the mean yield of the plots: divide the sum of all the yields in table 1 (965) by the total number of plots (30). Then use the mean yield (32) in this equation:

$$\text{Plot variability in percent} = \frac{\text{Plot variability x 100}}{\text{Mean yield}} = \frac{4.3 \text{ x } 100}{32} = 13\%$$

1/ George W. Snedecor, Statistical Methods, 4th ed., 1946, sec. 2.16.

Table 1. Survey data on yields of 2 adjoining plots on each of 15 farms. (Each plot is of the size to be used in the extensive test.)

Farm	Plot 1	Plot 2	Difference	Rank of difference
1	31	35	4	3
2	21	33	12	15
3	25	20	5	6
4	74	68	6	8
5	32	31	1	1
6	21	28	7	11
7	24	18	6	9
8	20	22	2	2
9	52	61	9	13
10	13	17	4	4
11	46	36	10	14
12	33	28	5	7
13	30	24	6	10
14	38	46	8	12
15	17	21	4	5

Total yield	965
Number of plots	30
Mean yield	32

Table 2. Relisting of farms of table 1 in order of rank.

Rank	Farm	Difference
1	5	1
2	8	2
3	1	4
4	10	4
5	15	4
6	3	5
7	12	5
8	4	6
9	7	6
10	13	6
11	6	7
12	14	8
13	9	9
14	11	10
15	2	12

Substitute this value for the one you got by the "guessing" procedure in Part III and use it in calculating the extensive-test error (Part III, p. 15).

Method 2

Method 2 calls for survey data and requires a calculating machine; use of the machine will help you obtain a more accurate measure of variability. Start with data like those in table 1, but omit the last column; instead, square each difference in the fourth column and add the squares:[2/]

$$4^2 + 12^2 + \ldots + 4^2 = 649$$

To find plot variability, divide this sum of squares by the number of plots, and extract the square root of the quotient:

$$\text{Plot variability} = \sqrt{\frac{\text{Sum of squares of differences}}{\text{Number of plots}}}$$

$$= \sqrt{\frac{649}{30}}$$

$$= \sqrt{22}$$

$$= 4.7$$

Plot variability in percent is obtained in the same way as for Method 1:

$$\text{Plot variability in percent} = \frac{4.7 \times 100}{32}$$

$$= 15\%$$

Bear in mind that use of a calculating machine does not of itself assure a reliable estimate of plot variability. The estimate is good only so far as the data are representative of all the farms in the area. You should therefore evaluate both the source of the survey data and the amount that is available. You must have measures of plot variability from a sufficient number of farms, preferably selected at random, before you can place much confidence in your mathematical estimate. You may even modify this estimate by judging the representativeness of the data, and so arrive at a better estimate. For instance, your data may have come from soils that are more uniform than other soils in the region and may, in your judgment, underestimate plot variability for the region as

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2/ Ibid., sec. 4.2.

a whole. If so, you might increase your calculated value by a small amount; thus, in our example, you might increase it from 15 percent to 20. The estimate you finally arrive at is the one to use in calculating the extensive-test error (Part III, p. 15).

Method 3

Method 3, which uses data from experiments and requires a calculating machine, gives the most exact estimate of variability. It is the method that you are likely to choose if you are working in an area that already has an agricultural research organization, for such organizations usually accumulate data of the kind required for the method. These accumulated experimental data, however, will be useful to you only if they have been collected from farms varied enough to be fairly representative of plot variability in the region. If they have not, you will do better to use data obtained from surveys and to analyze them by Methods 1 or 2. Above all, avoid the error of making a plot-variability estimate from experiments that were all conducted at one location, as at a research station.

If you are working in an area where research experiments are under way, you can enlist the assistance of the technician carrying out the experiments: he can obtain plot variability from the analysis of variance he makes of his data. Which item in his analysis is the measure of plot variability will depend on the experimental design he has used; one example, however, will suffice here to show how variability can be measured by analysis of experimental data.

Our example is based on a hypothetical experiment set up in a randomized block design, which is a rather usual type. The experiment tests four treatments, covers four farms, and has two replications, or blocks, per farm. 3/ Actual experiments may differ in design from this one, but all well-designed experiments have these characteristics: (1) two or more treatments, or factors, under test; (2) two or more replications of the treatments at each location; (3) repetition of the same or closely related treatments at several locations.

The first step in the analysis is to tabulate the data on yields from each plot by treatment and location and to add up the total yields for treatments, farms, and replications (table 3).

Next, prepare a worksheet like that shown in table 4; at the head of it note the total number of plots in the experiment (32) and the total yield from these plots (1,035). Now, using a calculating machine, proceed to get the various sums of squares, entering the figures for each step in the table.

3/ Snedecor, op. cit., sec. 11.13.

Table 3. Experimental data on yields of 32 plots in an experiment with 4 treatments, 4 farms, and 2 replications per farm. (Each plot is of the size to be used in the extensive test)

Treatment	Farm 1			Farm 2			Farm 3			Farm 4			Total
	Rep. 1	Rep. 2	Total	Rep. 1	Rep. 2	Total	Rep. 1	Rep. 2	Total	Rep. 1	Rep. 2	Total	
A	39	31	70	36	32	68	27	34	61	35	38	73	272
B(check)	34	35	69	27	24	51	26	21	47	32	33	65	232
C	47	39	86	37	39	76	30	39	69	42	41	83	314
D	36	25	61	26	23	49	21	36	57	21	29	50	217
Total	156	130	286	126	118	244	104	130	234	130	141	271	1035

Table 4. Worksheet for calculating sums of squares of data in table 3. (Number of plots, 32; sum of yields, 1,035)

Item	Total	Treatments	Farms	Treatments x farms	Replications on farms
Uncorrected sum of squares	34,903	273,493	269,529	69,063	135,533
Divisor	1	8	8	2	4
Quotient	34,903	34,187	33,691	34,532	33,883
Correction factor	33,476	33,476	33,476	33,476	33,476
Corrected sum of squares	1,427	711	215	1,056 - 711 - 215 130	407 - 215 192

The first one is designated as "total": it is the sum of the squares of each individual yield number shown in table 3. The uncorrected value is as follows:

$$39^2 + 34^2 + \ldots 29^2 = 34{,}903$$

Enter this value in the first line under the column headed "Total." Immediately beneath it enter the "Divisor," which is merely the number of plots included in each number you have just squared; in this case it is only 1. Divide the uncorrected sum of squares by this divisor to get the quotient 34,903. Before you can arrive at a corrected sum of squares, you have yet to compute a correction factor:

$$\text{Correction factor} = \frac{\text{Total yield}^2}{\text{Number of plots}}$$

$$= \frac{1{,}035^2}{32}$$

$$= 33{,}476$$

Subtract this correction factor from the quotient,

$$34{,}903 - 33{,}476 = 1{,}427,$$

and you have the corrected sum of squares to enter as the last figure in the column.

The second sum of squares is calculated from the <u>treatment</u> totals of table 3:

$$272^2 + 232^2 + 314^2 + 217^2 = 273{,}493$$

Since each treatment total includes the yields of 8 plots, the divisor is 8. From this point proceed as you did in finding the corrected sum of squares for the total, and you will arrive at the corrected sum of squares for treatment--711.

The third sum of squares is calculated from the <u>farm</u> totals in table 3:

$$286^2 + 244^2 + 234^2 + 271^2 = 269{,}529$$

Again the divisor is 8 since each farm total includes 8 plots. Proceeding as before, you obtain the corrected sum of 215.

The fourth sum of squares is calculated from the totals of <u>each treatment on each farm</u> in table 3:

$$70^2 + 69^2 + \ldots + 50^2 = 69{,}063$$

Since each squared number in this calculation includes 2 plots, the divisor is 2. Proceeding as before, you obtain the number 1,056. But this is not the corrected sum, as it would have been in the first 3 columns. From it you must first subtract the sums of squares for treatments (711) and for farms (215) to get 130, the sum of squares for treatments x farms.

The fifth sum of squares is calculated from the totals of each replication on each farm in table 3:

$$156^2 + 130^2 + 126^2 + \ldots + 141^2 = 135{,}533$$

The divisor is 4 since each replication total includes 4 plots. Now proceed as before until you obtain the value 407; this number, however, contains not only the replication differences but the farm differences as well. Hence subtract the sum of squares for farms (215); the remainder, 192, is the corrected sum of squares for replications on farms.

Now transfer these 5 sums of squares to the analysis-of-variance sheet (table 5). In the first column list the various sources of variation, i.e., the various factors that contributed to the yield: treatments, farms, treatments x farms, and replications on farms. For each of these, and for the total, sums of squares are entered (from table 4) in the second column. For the item just before the total, "treatments x replications on farms," the sum of squares is obtained by subtracting all other sums of squares from the total sum of squares.

The third column in table 5 is headed "degrees of freedom." For each source of variation the number of degrees of freedom is as follows:

Treatments: Number of treatments (4) less 1.
Farms: Number of farms (4) less 1.
Treatments x farms: Degrees for treatments (3) multiplied by degrees for farms (3).
Replications on farms: Number of replications on each farm less one (2 - 1) multiplied by number of farms (4).
Treatments x replications on farms: Total degrees (number of observations less 1, or 32 - 1) less the degrees found thus far (3 + 3 + 9 + 4).

In the last column are listed the mean squares. These are obtained by dividing the degrees of freedom into the sum of squares. For example, the mean square for "treatments x replications on farms" is 179/12, or 15.

Table 5. Analysis of variance of data in table 3, for arriving at an estimate of plot variability.

Source of variation	Sum of squares	Degrees of freedom	Mean square
1. Treatments	711	3	237
2. Farms	215	3	72
3. Treatments x farms	130	9	14
4. Replications on farms	192	4	48
5. Treatments x replications on farms (plot variability)	179	12	15
6. Total	1,427	31	--

You are now ready to calculate the plot variability:

$$\text{Plot variability} = \sqrt{\text{Mean square for "treatments x replications on farms"}}$$

$$= \sqrt{15}$$

$$= 3.87$$

To express this variability in percent, use the following equation, which calls for the mean yield of untreated plots. These plots are the ones that were shown in table 3 as receiving treatment B; i.e., the check treatment.

$$\text{Plot variability in percent} = \frac{\text{Plot variability x 100}}{\text{Mean yield of check plots}}$$

$$= \frac{3.87 \times 100}{29.0}$$

$$= 13\%$$

Use this estimate in the calculations of extensive-test error (Part III, page 15).

The tests at the different farms may not involve exactly the same experimental design. Often the treatments are not exactly the same. As long as they are reasonably similar, you can combine them, even if the number of treatments or replications differ. In order to combine plot variability from different experiments, calculate first the plot variability in percent for each experiment separately. Then square the percentages. Add the squares, and obtain the mean of the sum by dividing it by the number of experiments. Finally, take the square root of the mean. As an example:

Experiment	Plot Variability (Percent)	Squares
1	12	144
2	15	225
3	10	100
4	13	169
Sum of squares		638
Mean of squares (638/4)		160
Mean plot variability ($\sqrt{160}$)		12.6%

A word of caution about Method 3: The calculations are rather exact, but in using the resulting information for designing an extensive test, it is generally necessary to exercise some discretion as to applicability. If the plots to be used in the extensive test are much larger than those used in the experiment, additional calculations are necessary (see page 28).

Furthermore, in order to feel confident about using the information from experiments, you must be fairly sure that soil variability at the experiment stations is reasonably representative of soil variability on farms in the region. If the soil is more uniform at the experiment stations than it is likely to be on the extensive-test farms, you would be justified in raising somewhat the estimate of plot variability.

LOCATION VARIABILITY

Method 1

If survey data--the kind used for Method 1--do not already exist for your area, you can obtain some by making a quick survey of farms. An example of data collected in such a survey is shown in Table 6: 15 farms were selected at random, and yield per acre of the crop in question was obtained from the farms' total yield of that crop, not from small sample plots. If, however, you get yields from small plots, follow instead the procedure in Method 2 (page 18).

The first step in determining location variability from the yield data in table 6 is to add all the yields and divide by the number of locations, thus arriving at the mean yield--in this example, 32. Now find the difference between the mean yield and each location yield, and enter these differences, or deviations, in the third column. Rank the locations in increasing order of the deviations: Farm 2 has the smallest deviation and ranks first; Farm 4 has the largest deviation and ranks fifteenth. Now list the 15 farms in the order of this rank (table 7). The midpoint, or median, rank is 8; and the deviation corresponding to this rank is 9. Location variability is now obtained as follows:

$$\text{Location variability} = \frac{\text{Deviation for midpoint rank}}{0.7^{4/}}$$

$$= \frac{9}{0.7}$$

$$= 13$$

To express the variability in percent, use this equation:

$$\text{Location variability in percent} = \frac{\text{Location variability x 100}}{\text{Mean yield}}$$

$$= \frac{13 \text{ x } 100}{32}$$

$$= 41\%$$

Use this estimate in the calculations of extensive-test error (Part III, page 15).

4/ Snedecor, op. cit., sec. 2.16.

Table 6. Survey data on average yield of entire farms, for 15 randomly selected farms.

Farm	Yield per acre	Deviation from mean	Rank
1	27	5	4
2	33	1	1
3	23	9	7
4	71	39	15
5	31	1	2
6	24	8	6
7	21	11	10
8	21	11	11
9	57	25	14
10	15	17	13
11	41	9	8
12	31	1	3
13	27	5	5
14	42	10	9
15	19	13	12
Total yield	483		
Mean yield	32		

Table 7. Relisting of farms of table 6 in order of rank.

Rank	Farm	Deviation
1	2	1
2	5	1
3	12	1
4	1	5
5	13	5
6	6	8
7	3	9
8	11	9
9	14	10
10	7	11
11	8	11
12	15	13
13	10	17
14	9	25
15	4	39

Method 2

With Method 2, which calls for a calculating machine, you can make a more accurate estimate of location variability from the survey data. We will illustrate the procedure for two types of data: one in which yields of entire farms are available, and the other in which yields of sample plots are available.

The data in table 6 can be used as an example of yields for entire farms. Start with the yields in the second column of the table and add their squares:[5]

$$27^2 + 33^2 + \ldots + 19^2 = 18{,}777$$

From this value, the uncorrected sum of squares, subtract a "correction factor" obtained by squaring the sum of the yields and dividing by the number of farms:

$$\text{Correction factor} = \frac{483^2}{15}$$

$$= 15{,}553$$

Subtracting 15,553 from 18,777, you get 3,224. This value is called the corrected sum of squares. Now divide by the number of farms less 1, that is, by 15 - 1, to get the mean square:

$$\text{Mean square} = \frac{\text{Sum of squares}}{\text{Number of farms} - 1}$$

$$= \frac{3224}{14}$$

$$= 230$$

The square root of 230, or 15, is the location variability. Express it in percent as follows:

$$\text{Location variability in percent} = \frac{\text{Plot variability} \times 100}{\text{Mean yield}}$$

$$= \frac{15 \times 100}{32}$$

$$= 47\%$$

5/ Snedecor, op. cit., sec. 2.8.

This 47 percent is a more exact computation of the location variability than the 41 percent obtained by Method 1. This becomes, then, the estimate to use in calculating extensive-test error (Part III, page 15).

Now let us illustrate the procedure when the yields are for small plots on farms rather than for entire farms.[6] You must measure 2 plots side by side on each farm. Hence, you can use here the data collected for plot variability in table 1. However, summarize the data as shown in table 8.

The first step is to square each observation, or the value for each plot, and add the squares to get the uncorrected sum:

$$31^2 + 21^2 + \ldots + 21^2 = 37{,}769$$

Then compute the correction factor:

$$\text{Correction factor} = \frac{\text{Total yield}^2}{\text{Number of plots}}$$

$$= \frac{965^2}{30}$$

$$= 31{,}041$$

The difference between the uncorrected sum of squares and this correction factor, 37,769 - 31,041, or 6,728, is called the total sum of squares. Enter it in the indicated place in table 9. In the next column, enter the total degrees of freedom--the total number of observations less 1, or 30 - 1.

The next value to compute is the sum of squares for farms. Add the squares of the farm totals (in last column of table 8) and divide by the number of plots per farm:

$$\frac{66^2 + 54^2 + \ldots + 38^2}{2} = \frac{74{,}889}{2} = 37{,}444$$

and then subtract the correction factor, already computed as 31,041, to get 6,403. Enter this value in table 9 as the sum of squares for farms. In the next column enter the degrees of freedom--the number of farms less 1, or 15 - 1.

Now, for plots, obtain the sum of squares and degrees of freedom by subtracting the values for farms from the values for total.

6/ Snedecor, op. cit., sec. 10.6.

Table 8. Survey data on yields of 2 adjoining plots on each of 15 farms. (Each plot is of the size to be used in the extensive test)

Farm	Plot 1	Plot 2	Sum
1	31	35	66
2	21	33	54
3	25	20	45
4	74	68	142
5	32	31	63
6	21	28	49
7	24	18	42
8	20	22	42
9	52	61	113
10	13	17	30
11	46	36	82
12	33	28	61
13	30	24	54
14	38	46	84
15	17	21	38

Total yield 965

Number of plots 30

Mean yield 32

Table 9. Analysis of variance of data in table 8, for arriving at location and plot variabilities.

Source of variation	Sum of squares	Degrees of freedom	Mean square	Units	Variance component
Farms	6,403	14	457		
Plots	325	15	22	1	22
Total	6,728	29			
Difference due to farms			435	2	218

Obtain the mean squares for farms and plots by dividing the degrees of freedom into the sum of squares. Then, to find out what variation was due to the farms, subtract the mean square for plots from the mean square for farms. You have to do this to eliminate the plot-to-plot variation from the farm differences.

The next column of table 9, "units," gives the number of individual plots that were represented in the numbers you squared. For farms, you squared a value that represented 2 plots; hence, enter 2 in the bottom line of that column. For plots, you squared the values for individual plot yields; hence, enter 1 for plots. Divide the mean squares by the respective number of units to get the values called variance components. The square roots of these variance components are the variability values we seek. For farms (locations) the variability is the square root of 218, or 14.8. Expressed in percent of the mean, it is:

$$\text{Location variability in percent} = \frac{\text{Location variability} \times 100}{\text{Mean yield}}$$

$$= \frac{14.8 \times 100}{32}$$

$$= 46\%$$

Use this variability in calculating extensive-test error (Part III, page 15).

If the plots of the survey are approximately the same size as those you will use in the extensive test, the variance component for plots can be used to determine plot variability. Follow the same procedure as with location variability:

$$\text{Plot variability} = \sqrt{22}$$

$$= 4.7$$

You express this variability in percent as follows:

$$\text{Plot variability in percent} = \frac{\text{Plot variability} \times 100}{\text{Mean yield}}$$

$$= \frac{4.7 \times 100}{32}$$

$$= 15\%$$

Note that you have the same answer here as you obtained by Method 2 for plot variability, page 6. Use this estimate in your calculation of extensive-test error (Part III, page 15).

Method 3

Method 3, which uses experimental data for estimating variability, should be used for location variability only if the experiments have been conducted on enough farms to constitute a fair sampling of the region. A minimum of 10 farms is a good rule-of-thumb requisite. Most experiments are conducted in only one location, or in only a few, and therefore cannot serve the purpose.

Table 3 shows experimental data from only 4 farms, not enough to make a reasonably precise estimate of location variability. When data are from so few locations, do not use Method 3 for calculating location variability. Instead, use one of the other methods already described. They are based on a more inclusive sample and also require much less computation.

Another disadvantage of using experimental data for determining location variability is that the calculations are usually complicated. You will have to call on the technicians at the research station to do the calculations. This may be feasible, and we will therefore give an example of the procedure in the next section, on treatment variability. In that section we will use the same example for both treatment and location variability.

TREATMENT VARIABILITY

Only data from experiments can be used to calculate treatment variability. Hence, Methods 1 and 2, the ones that work with survey data, are not applicable. But, in using Method 3, be sure that your data are from an experiment that meets the following requirements: (1) Experimental farms must be representative of the region as to both anticipated treatment effects and treatment variability and (2) experimental treatments must be similar to those planned for the extensive test. Unless the experiment meets both these requirements, you had better use the method given in Part III (page 13) of this Guide.

Let us assume that the data in table 3 are from an experiment that meets these requirements and therefore can be used to illustrate the procedure of Method 3. Start with the analysis of variance that was made of the data in table 5.[7/] [8/] You now rewrite the table, using symbols as shown in table 10. The purpose of the revision is to help

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7/ William G. Cochran and Gertrude M. Cox. Experimental Designs. New York, 1950, Sec. 14.1.

8/ Snedecor, op. cit., sec. 11.13.

you isolate the variance component for treatment variability from the rest of the information obtained in the experiment. The revision looks imposing but is not difficult if you follow directions step by step.

Copy the first column of table 5 into the first column of table 10. Filling in the next section of the table, "Calculating mean square in symbols," is accomplished in 3 steps that will be easy if you simply follow directions. By the time you have finished the third step, you will have written equations that show the various components in symbols for each mean square.

Step 1 is to assign a letter as a symbol for each source of variation thus: T for treatments; F for farms; $R_{(F)}$ for replications, or plots on farms; and all the symbols--$TFR_{(F)}$ --for the total. After each symbol, write the number that applied in the experiment: table 3 will remind you that there were 4 treatments, 4 farms, and 2 replications per farm, making 32 plots in all.

In Step 2, write the cofficient for each variance in step 1. This consists of the symbols that are missing. To clarify: all the symbols, T, F, and $R_{(F)}$, are found in the total; but not all are found in each variance. Whichever ones are missing in each are now to be written under Step 2, in small letters. Thus, for variance T, both F and $R_{(F)}$ are missing; write them so--$fr_{(f)}$. When you get down to the fourth line, note that the variance, $R_{(F)}$, lacks both T and F; but the F is not written in Step 2 because there is a rule that when a symbol appears in parenthesis in the variance of Step 1, this symbol shall not be repeated in the coefficient.

Now you come to Step 3, which, when finished, will give you the complete formulas. First copy each variance into this column, preceding it with its coefficient; for "treatments," for instance, write $fr_{(f)}T$. Now add to it all other variances that also contain the identifying symbol T, not forgetting to precede each variance with its coefficient. In this case, the other variances that contain T are TF and $TR_{(F)}$; the latter, not having a coefficient to precede it, is added all by itself. Continue thus to the end of the column. When you get to the last line, all you will have to enter will be $TR_{(F)}$, for there is no other variance with all these symbols, neither does it have a coefficient to precede it.

Having calculated the mean squares in symbols, copy in the last column the numerical mean squares from table 5; and then you have everything you need to solve the symbol equations for the numerical value of the variance components. Start with $TR_{(F)}$, the plot variability on the fifth line. You need do no more than look in the last column to find that its value is 15. Now you can solve for $R_{(F)}$ on line 4:

$$tR_{(F)} + TR_{(F)} = 48$$

Table 10. Revision of table 5 to obtain variance components for determining variability by Method 3.

Source of variation	Calculating mean square in symbols			Mean square in numbers
	Step 1 (Variance)	Step 2 (Coefficient)	Step 3 (Completed formula)	
Treatments	T (4)	$fr_{(f)}$	$fr_{(f)} T + r_{(f)} TF + TR_{(F)}$	237
Farms (location variability)	F (4)	$tr_{(f)}$	$tr_{(f)} F + r_{(f)} TF + tR_{(F)} + TR_{(F)}$	72
Treatments x farms (treatment variability)	TF	$r_{(f)}$	$r_{(f)} TF + TR_{(F)}$	14
Replications on farms	$R_{(F)}$ (2)	t	$tR_{(F)} + TR_{(F)}$	48
Treatments x replications on farms (plot variability)	$TR_{(F)}$	-	$TR_{(F)}$	15
Total	$TFR_{(F)}$ (32)			

Since $t = 4$ (from the second column) and $TR_{(F)} = 15$, rewrite the equation thus:

$$4R_{(F)} + 15 = 48$$

$$R_{(F)} = \frac{48 - 15}{4}$$

$$= 8.25$$

Now proceed to solve for treatment variability, on the third line from the bottom:

$$r_{(f)}\,TF + TR_{(F)} = 14$$

$$2TF + 15 = 14$$

$$TF = \frac{14 - 15}{2}$$

$$= -0.5$$

This negative value is unusual; generally TF is positive. A negative variance component should be taken as zero.

When you obtain a positive value for TF--25, for example--simply take its square root to obtain the treatment variability:

$$\text{Treatment variability} = \sqrt{TF}$$

$$= \sqrt{25}$$

$$= 5$$

To express this treatment variability in percent, use the following equation, which calls for the mean yield of check, or untreated, plots (see table 3 for yields of plots receiving treatment B):

$$\text{Treatment variability in percent} = \frac{\text{Treatment variability x 100}}{\text{Mean yield, check plots}}$$

$$= \frac{5 \text{ x } 100}{29}$$

$$= 17\%$$

Use this estimate in your calculation of extensive-test error (Part III, page 15).

While we were discussing plot variability early in this section, we pointed out that experimental data can be used for estimating location variability also, provided the experiment covers enough locations to provide an adequate sample. The experiment summarized in table 3 had only 4 locations, hardly enough for a region of any size. Nevertheless we can use this experiment to illustrate the procedure for determining location variability. Refer again to table 10, and write the equation for farms:

$$tr_{(f)}F + r_{(f)}TF + tR_{(F)} + TR_{(F)} = 72$$

Substitute the coefficients and the variabilities already solved:

$$(4 \times 2F) + (2 \times 0) + (4 \times 8) + 15 = 72$$

$$8F + 0 + 32 + 15 = 72$$

$$F = \frac{72 - 32 - 15}{8}$$

$$= \frac{25}{8}$$

$$= 3.12$$

Then, since location variability is simply the square root of F--

$$\text{Location variability} = \sqrt{F}$$

$$= \sqrt{3.12}$$

$$= 1.77$$

To express this location variability in percent, use this equation:

$$\text{Location variability in percent} = \frac{\text{Location variability} \times 100}{\text{Mean yield of check plots}}$$

$$= \frac{1.77 \times 100}{29}$$

$$= 6\%$$

This is the estimate that you will use in calculating extensive-test error (Part III, page 15).

MODIFYING THE ESTIMATE FOR LARGE PLOTS

If you have determined plot variability from data taken from plots of the same size as those you will use in the extensive test, you will not need the information in this section. But you may wish to use larger plots in the extensive test, especially in order to enhance its demonstrational value. If so, you will need the information given here.

When the extensive-test plots are to be much larger than the experimental plots, there is an additional adjustment to make. The procedure is quite simple. If the experimental plots were 5 square feet and the extensive-test plots will be 500 square feet, each of the latter will contain 100 experimental-plot units. Now refer to table 11. In the first column are listed a range of ratios between the sizes of the extensive-test plots and the experimental plots. The remaining columns give corresponding factors: one for crop tests, the other for animal tests. For a plot-size ratio of 100, the factor for crop tests is 3.2. Now, if the plot variability in the experimental plots is 13 percent (for example, page 13), then--

$$\text{Plot variability for extensive test (\%)} = \frac{\text{Plot variability in experiment (\%)}}{\text{Factor}}$$

$$= \frac{13}{3.2}$$

$$= 4\%$$

The plot variability thus obtained is the one to use in calculating extensive-test error (Part III, page 15).

A word of explanation will clarify the two sets of factors in table 11. In crop tests, the plot variability does not decrease in proportion to the size of the plots: as the size of the plots is increased, more variable ground is likely to be included. The variability between two plots lying side by side is less than the variability between two plots some distance apart. Thus, the set of factors for field crops, which is the fourth root of the ratios shown in the first column, takes into consideration this increased variability of soil with increasing plot size.[9] In animal tests, on the contrary, this circumstance does not apply; therefore, the factors used are simply the square roots of the ratios shown in the first column. The factors for animal tests can be used also for any other tests in which increased "plot size" does not mean a proportionate increase in the area of land per plot.

9/ An adaptation from "An Empirical Law Describing the Heterogeneity in the Yields of Agriculture Crops," by H. Fairfield Smith, Jour. Agric. Sci. 28(Part 1):1-23, January 1938. In table 11, an average heterogeneity factor of 0.50 is used.

Table 11. Factors to be used in determining the plot error when the extensive-test plots are larger than the experimental plots on which estimate of variability was based.

Ratio: Extensive-test plot size / Experimental plot size	Factor for crop tests 1/	Factor for animal tests 2/
1	1.0	1.0
2	1.2	1.4
3	1.3	1.7
4	1.4	2.0
5	1.5	2.2
6	1.6	2.4
8	1.7	2.8
10	1.8	3.2
20	2.1	4.5
30	2.3	5.5
40	2.5	6.3
50	2.7	7.1
100	3.2	10.0

1/ This factor is the fourth root of the number in the first column: $\sqrt[4]{1}$, $\sqrt[4]{2}$, etc.

2/ This factor is the square root of the number in the first column: $\sqrt{1}$, $\sqrt{2}$, etc.

DETERMINING THE BEST SIZE OF PLOT

Most often you will not be concerned with the best size of the plots for an extensive test. Sometimes, however, it is necessary to limit the size of the plots as much as possible. For example, in a test of a new insecticide, you may not be able to get a sufficient quantity to permit large plots in the test. The problem of best plot size arises also when you are testing large things--trees, fruits, animals, or even people. You may want, then, to use the smallest plot that you can. In this section a procedure will be given to help you determine the best number of individuals, or units, to have in a plot.

You recall from Part III of this Guide, page 15, that the error for an extensive test is the sum of several variability components. But, if we have a small number of individuals in a plot, the variability of these individuals also becomes important, and it, too, must be added to the other variabilities in our error calculations. You will now see how to do this.

First, clearly designate the individuals, or units, that you are dealing with--individual plants and trees, for example, or short lengths of row in field crops, single hills with several plants per hill, individual animals in cattle experiments, small quadrats in pasture and forage experiments, students in a school, persons in a family, or farmers in a community.

Next, obtain an estimate of the variability of these units. The value you seek is the variability among units receiving no experimental treatment. The procedure you follow is the same as the one we have discussed for determining plot variability except that now you are dealing with differences between individuals instead of with differences between plots. The question you must answer is this: "If I took 2 units per plot at random at a number of farms in the region, what difference in yield would there be between them?" This question may be answered by judgment (Part III, pages 13-14) or from survey data (see the section on plot variability in this Part, Methods 1 and 2, pages 3-7).

This question may be answered also by experimental data, but then a somewhat different procedure is used. To begin with, get measures of individual, or unit, yields in the experiment as well as yields of the whole plots. In table 3, for instance, yields are given for the plots as a whole, but these must now be supplemented with data for units. Such data can be gathered by harvesting either every unit (plant, hill, etc.) or just two units at random in 20 or so of the plots. You will probably do the latter since it is less work, and we will therefore show this procedure in detail.

Let us start with the experiment shown in table 3 and assume that each plot contains 27 plants. Now, go into 20 of these plots and, from each, harvest 2 plants at random. List the yield data as in table 12. Get the difference between each pair of plants and write the differences in the last column of the table.

Now square each difference, add the squares, divide by 2 (the number of plants per plot actually measured) and multiply by 27 (the total number of plants per plot). This calculation gives the sum of squares for plants in plots:

$$\text{Sum of squares (plants in plots)} = \frac{(1.01^2 + 0.14^2 + \ldots + 0.21^2)27}{2}$$

$$= 57.7287$$

Next make the analysis of variance of data as it is shown in table 13. For plots, the values can be merely copied from the plot-variability line in table 5. For plants within plots, insert values as follows:

Sum of squares = the sum of squares you have just now computed.

Degrees of freedom = the number of differences listed in table 12.

Mean square = the quotient resulting from dividing the sum of squares by the degrees of freedom.

Square root of mean square = $\sqrt{2.89}$, or 1.70.

Units = the number of plants in each plot.

Factor = the fourth root of the number of units, since this is a crop test (see table 11, second column).

You have yet to enter the values of differences due to plots alone, which are as follows:

Mean square = the difference between mean square for plots and the mean square for plants in plots = 14.9 - 2.89 = 12.0.

Square root of mean square = $\sqrt{12}$, or 3.46.

Units = 1

Factor = the fourth root of the number of units (see table 11, second column).

Table 12. Yields of 2 plants selected at random from a total of 27 plants in each of 20 plots of the experiment shown in table 3.

Plot	Plant 1	Plant 2	Difference
1	0.67	1.68	1.01
2	1.37	1.23	0.14
3	0.94	1.26	0.32
4	1.51	0.79	0.72
5	1.66	1.04	0.62
6	1.25	1.21	0.04
7	1.39	0.84	0.55
8	1.27	1.39	0.12
9	1.28	1.10	0.18
10	1.20	1.16	0.04
11	1.19	1.07	0.12
12	1.28	1.19	0.09
13	1.00	1.43	0.43
14	1.76	1.03	0.73
15	1.11	1.50	0.39
16	1.58	1.13	0.45
17	1.30	1.24	0.06
18	1.04	1.55	0.51
19	1.21	1.90	0.69
20	0.99	0.78	0.21

Table 13. Analysis of variance of data for 20 plots and for 2 plants within each plot, with 27 plants per plot.

Source of variation	Sum of squares	Degrees of freedom	Mean square	Square root of mean square	Number of units	Factor
Plots	179	12	14.9			
Plants within plots	57.7287	20	2.89	1.70	27	2.28
Difference due to plots			12.0	3.46	1	1.00

The variabilities are obtained by multiplying the square roots of the mean squares by the factors in the last column of table 13:

$$\text{Plant variability} = 1.70 \times 2.28$$
$$= 3.88$$
$$\text{Plot variability} = 3.46 \times 1.00$$
$$= 3.46$$

These variability components are now to be expressed in percent. Simply multiply the variability by 100 and divide by the mean yield for the check plots. This mean yield is obtained from table 3, which indicates the check plots as those receiving treatment B; divide the total yield from these plots (232) by the number of plots (8). Then--

$$\text{Plant variability in percent} = \frac{\text{Plant variability} \times 100}{\text{Mean yield (check plots)}}$$
$$= \frac{3.88 \times 100}{29}$$
$$= 13.4\%$$
$$\text{Plot variability in percent} = \frac{\text{Plot variability} \times 100}{\text{Mean yield (check plots)}}$$
$$= \frac{3.46 \times 100}{29}$$
$$= 11.9\%$$

Now we are ready to determine the error of the extensive test. You do this by adding the variability components. You have just found the components for plants and plots; let us assume values of 46 percent and 10 percent respectively for the location and treatment components in order to illustrate the rest of the procedure. It is like the one shown in Part III, page 15, with the addition of the plant component and the factor (F) for the number of plants per plot, which you obtain from table 11. For Plan A use all four components (the various plans are given in Part III, in the Appendix):

Plant variability	= 13/F%; squared =	$(13/F)^2$
Plot variability	= 12%; squared =	144
Location variability	= 46%; squared =	2,116
Treatment variability	= 10%; squared =	100
Total of the squares		$(13/F)^2 + 2{,}360$
Extensive-test error		$\sqrt{(13/F)^2 + 2{,}360}$

For Plans B to H, omit the location variability:

$$\begin{array}{lll}
\text{Plant variability} = 13/F\%; & \text{squared} = & (13/F)^2 \\
\text{Plot variability} = 12\%; & \text{squared} = & 144 \\
\text{Treatment variability} = 10\%; & \text{squared} = & \underline{100} \\
\quad \text{Total of the squares} & & (13/F)^2 + 244 \\
\quad \text{Extensive test error} & & \sqrt{(13/F)^2 + 244}
\end{array}$$

Now, set up table 14 to aid in determining the best number of units per plot. Record first the specifications of the extensive test. The first column in table 14 indicates the plan. The second column lists various numbers of units per plot, a range from 2 to 40. You can try whatever numbers you are interested in. In column 3, the units are transposed into factors by referring to table 11. Since this is an experiment with a crop, the factors in the second column of table 11 are used. For tests in which the plots are not units of land, the factors in column 3 of table 11 would be used. Column 4 is the plant variability (13 percent) divided by the factors of column 3. Column 5 is the square of column 4. Column 6 is column 5 plus the remainder of the error shown at the head of the table: 2,360 for plan A and 244 for plans B to H. Column 7 is the square root of column 6; these values are the errors for the plans. Column 8 is the difference to be tested, shown at the head of the table as 30 percent, divided by the plan errors shown in column 7. In column 9 record from Part III, table 2, the number of replications required for the values in column 8.

Column 9 gives the answer you seek. For plan A note that the number of replications required is rather large and does not decrease beyond 5 plants per plot. Hence, 5 plants per plot are ample if you desire to keep the plots as small as possible.

For plans B to H the number of replications required is much fewer. They decrease up to 10 plants per plot but not thereafter. You have little to gain, then, by having more than 10 plants in a plot.

Once you have selected the minimum number of plants per plot--5 for plan A, and 10 for plans B to H--go back to Part III, table 3. Enter the corresponding number of replications in column 4 of that table. Then complete the rest of the summary in that table to determine the total requirements of the extensive test with the stipulated number of plants per plot.

Table 14. Example of a summary to aid in determining the minimum number of plants per plot for several designs.

SPECIFICATIONS

Number of treatments: 7 (1 is a check)
No. of plots on each farm: 1, 2, 3, or 4
Minimum difference: 30%
Anticipated variability components: Plants, 13%
Plots, 12%
Locations, 46%
Treatments, 10%

$$\text{Error (Plan A)} = \sqrt{(13/F)^2 + 2{,}360}$$

$$\text{Error (Plans B to H)} = \sqrt{(13/F)^2 + 244}$$

(1) Plan	(2) Units per plot	(3) Factor	(4) Plant variability ÷ Col. 3	(5) Column 4 squared	(6) Remainder of error + Col. 5	(7) Error % (Col. 6)	(8) Difference ÷ error (Col. 7)	(9) Replications required (Part III, Table 2)
A	2	1.2	10.8	117	2477	49.8	.60	60
	5	1.5	8.7	76	2436	49.4	.61	58
	10	1.8	7.2	52	2412	49.1	.61	58
	15	2.0	6.5	42	2402	49.0	.61	58
	20	2.1	6.2	38	2398	49.0	.61	58
	30	2.3	5.7	32	2393	48.9	.61	58
	40	2.5	5.2	27	2387	48.9	.61	58
B to H	2	1.2	10.8	117	361	19.0	1.58	11
	5	1.5	8.7	76	320	17.9	1.68	10
	10	1.8	7.2	52	296	17.2	1.74	9
	15	2.0	6.5	42	286	16.9	1.78	9
	20	2.1	6.2	38	282	16.8	1.79	9
	30	2.3	5.7	32	277	16.6	1.81	9
	40	2.5	5.2	27	271	16.5	1.82	9